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Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 7796; Fri, 01 Sep 89 00:28:50 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Mon, 09 Jan 89 00:28:35 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA04925; Fri, 1 Sep 89 00:29:07 +0200
Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA04040; Fri, 1 Sep 89 00:19:21 +0200
Message-Id: <8908312219.AA04040@cernvax.uucp>
Date: Fri, 1 Sep 89 00:17 GMT +1
From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
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Dear E632 and WA84 Colleagues,

25 August 1989.

COLD FUSION NEWS No. 19

Cold Fusion Heats Up! New Institutes Founded.

1. SUMMARY

082589 DROM

The last week has been rather wild and quite fascinating.

It might be thought that the recommendation of the Interim report of the DOE Panel on Cold Fusion "against any significant expenditures to establish cold fusion research centres" might have some effect. But the State of Utah has now set up the National Cold Fusion Research Institute after its Advisory Board appeared to have heard only positive testimony. Strong additional funding is expected from industry. Neighbouring BYU does not seem to have been involved, but they have now established their own Fusion Research Centre!

The menace of Japan taking over a great American discovery has been brandished. Now it is reported that a Japanese Institute of Fusion Science is being organised. Also in India a five foot tall cathode is being built which would be bigger than any other.

Then yesterday there was quite a bit of excitement in Utah when Mike Salamon gave a press conference to announce their results (see No.18), that their neutrons and gamma counters see no fusion from the cells in Dr. Pons's lab. The response of Dr. Pons was breath-taking (section 6).

Much of the justification of the Utah State Advisory Board came from the positive results obtained in Florida and at Texas A&M. However the Florida results are contested by a group upstairs(section 7.1) and the lack of checks for the Texas results are discussed in Section 7.2. Finally all positive and negative results are briefly summarised in Section 7.5

Comments from Utah (and elsewhere) on the DOE panel report are fine examples of contemporary politics and worth savouring (section 8.3).

2. STARTING UP OF NATIONAL COLD FUSION RESEARCH INSTITUTE

2.1 Introduction

A major new Research Institute has been set up which is intended to be National, not for the State of Utah only. The funding is expected to be national also. How this came about despite the recommendations of a federal panel, is a classic case worthy of study by students of Science Politics and of Life. The essential seems to be to reinforce the positive and exclude the negative - peer review is excluded.

2.2 Procedings and Vote of Advisory Board

The Utah State Advisory board on Energy/Fusion held hearings to establish whether the original claims of Profs Fleischmann and Pons were confirmed. All accounts agree that only positive results were presented. As they appeared impressive, it is only fair to the board members if we give them.

B. Stanley Pons told the Board that he had used "Cold Fusion" to boil water and suggested that household uses for his purported discovery were in the offing. In his experiment a "boiler" the size of a thermos emmitted 15 to 20 times the amount of heat being put into it -- a reaction that cannot be explained by normal chemical reactions, Dr. Pons said (Rayleigh Times and Observer, July 12). On the other hand it was said by GE that Drs. Pons and Fleischmann have admitted that their early data on gamma ray spectroscopy was flawed. It was said the results were " inappropriately homogenised" (SLC Tribune).

A report from the U. to the state Energy/Fusion Advisory Council was leaked to the Salt Lake City Tribune. It lists the "silent" organisations that have obtained confirmation of Fleischmann and Pons's work but have not published.

It said that scientists at the General Electric Company's "research facility at Schenectady, NY had reproduced the experiment and have 'obtained excess energy at about the 15% level'. The report also says that GE 'after long and careful study..... concludes that the basic calorimetric theory of Pons and Fleischmann is correct and shows excess energy'. GE refused to confirm the report. 'We're not confirming or disclaiming heat', said Peter Van Avery, GE's manager of communications [an interesting title - does it mean manager of telecommunications or of public relations? - my comments are in square brackets] at Schenectady. 'We're not making any claims at all, but the research is continuing.'"

" A scientist from GE also examined the research of (U College of Mines Dean)

Dr. Wadsworth, the report said. 'The GE thermodynamist concluded that those data cannot be explained by any error in measurement, nor any chemical phenomenon'"

The Deseret News later said that "The GE scientists also have reported finding significant levels of tritium in the electrolyte, but no signs of helium 4 in the bar".

"The report also said Edmund Storms and Carol Talcott of Los Alamos National Laboratory have electrochemical cells that produced excess heat 'after being coated with a lithium-palladium alloy'" [probably it was the electrodes that were coated].

"Drs. Storms and Talcott had earlier reported the production of tritium, a fusion by-product, in the same cells. Both researchers were unavailable for comment Tuesday".

"Charles D. Scott of Oak Ridge National Lab. has one cell that is showing 'small excess of heat(about 8%)', the report said. Dr. Scott was not in his office Tuesday afternoon, but Oak Ridge spokesman Ed Aebischer said 'He has apparently seen some evidence of heat production'. Mr. Aebischer added that the results were preliminary".

"More confirmation of heat was attributed to Michael Mackubre, a researcher at the Stanford Research Institute in Menlo Park, California. 'He has had several incidents of extended excess heating', the report said"

"The report said Dr. Mackubre used closed system calorimetry. Some critics of the U. research have said that a closed system, where none of the evolving gases is allowed to escape the cell, is more accurate than the open system used by Dr. Pons and the others who have reported the heat."

"Dr. Mackubre was unavailable but the results were acknowledged by a representative of for the Electrical Power Research Institute, which is funding the research."

"'That is correct', said John Maulbetsch, senior adviser in the office of exploratory research at EPRI. EPRI is a non-profit research consortium funded by electrical utility companies. 'There have been some indications of heat, but they have been sporadic'."

"And another scientist at Case Western Reserve University reported 'low levels of excess energy'. The researcher was identified only as E. Yeager."

"The report also made reference to instances of tritium production by Thomas Furtek at the University of Colorado. Tritium has been reported by a number of laboratories, but it has come in levels far below the excess heat."

The U student newspaper, the Summer Chronicle, reports some of the testimony of Drs Pons and Huggins. "Pons said one cell he has been experimenting with produced as much as 22 Watts of continuous power. 'Any reasonable engineer would be able to upscale (the experiment) for commercial use', Pons said. For example, the first commercial product will most likely be a typical water heater for a family home."

He said they would be submitting two papers in the next few months [elsewhere it is suggested that the first will be in September but in talking to Martin Fleischmann yesterday, he described to me so many calculations and other work that he would like in the paper, that it could take longer to write and to review]. "The first paper will detail the calorimetric measurements of the experiment and will present error limits for each calculation." It will perhaps be submitted to the Journal of Electroanalytical Chemistry [it will be interesting to see if it is reviewed the same way as their first paper which was strongly criticised - for example it is the first paper on calorimetry that I have seen that did not give a single temperature]. "The second will analyze the compound tritium which has been detected in small amounts. Pons said tritium is not found naturally and is only present after a nuclear reaction."

"Huggins said his fusion experiments have also produced excess heat and critics of the U experiment are not performing the thermal measurements correctly. 'At Stanford we have four sets of cells operating and each one has given off heat. But the heat from the experiments is giving very sporadic results. This shows that there are still a lot of questions to be answered' Huggins said."

On 21 July the Fusion/Energy Advisory Council voted unanimously to accept the work of B. Stanley Pons and Martin Fleischmann as confirmed. In view of the evidence presented above to the Council and the absence of any report of all the other experiments which found no effect, it was reasonable for the council to vote this way. However the Salt Lake Tribune noted that "it withheld approval of budget specifics because of concern over administrative costs".

"The fusion council's executive committee expedited its approval.... upon learning that the fusion race was gaining speed - particularly at the University of Florida where the most favourable confirmation of the U experiment was recently announced. Southern scientists said Gamma rays, tritium, and excess heat sufficient to light a light bulb, have been produced" (Deseret News).

On 7 August the state council voted to release $4.5 million for research into cold fusion and this allows the funding of a National Cold Fusion Institute for Research. The vote was 7 to 1 plus one abstention - he was Wilford Hansen, the only physicist on the panel, who had caused some controversy by staying on the

the panel although at the last moment he had applied for $750 000 of this money for his research. The negative vote was believed to be Dr. Karen Morse who is Dr. Hansen's Provost at USU who was particularly worried about the high administative costs. These two are the only scientists on the panel.

State officials said that this money represents one-third of the State's annual investment in economic development (Deseret News).

U officials said the initial staff will be composed of U faculty members supplemented by interested faculty from Utah State University, BYU, and visiting researchers from other universities and industrial labs. A permanent staff will be developed as quickly as funding levels and the recruiting process permits (Deseret News).

Someone who has studied the State bill to fund the research says that it is carefully written so that funding could be given even though the effect is not fusion.

2.3 Funding of The National Institute

A 25 000 square foot facility has been taken over in the University's Research Park and apparently the first employees were hired some time ago. There is a five year funding plan. Of the $4.5 million voted by the Utah State legislature, between $2.8 million (75% of the Institute's budget) will be spent the first year and the remaining $1.7 m (37%) in the following year. This is regarded as seed money and it is expected that a further $20 million will be forthcoming from industry and government sources. Dr. Pons has said that he will not be joining the Institute as he wishes to concentrate on experiments he should do and wants to do. Some Council members were worried that Drs Pons and Fleischmann would not be adequately funded but Dr. Brophy said "Dr. Pons will get whatever he wants" (Deseret News). He has received funding from the Office of Naval Research (SL Tribune).

The Electrical Power Research Institute, EPRI, is a group sponsored by many members of the electrical power industry. Its leaders and members appear to be believers in the production of excess heat, though they are cautious about other claims. While it is expected that government agencies will fund, this will take some time; the EPRI will fund fairly rapidly, in a few months. The local power utility, Utah Power and Light, is not a member of EPRI, though they follow cold fusion - but they look at ALL the data, both positive and negative.

The Deseret News of 8 August reported that GE has signed a financial agreement to develop fusion research. U. President Chase Peterson said that GE's investment would be "sizable". Dr. James Brophy told the Council that additional corporate funding, likely to include Westinghouse, is being sought and eventually funding from the DOE is anticipated. In this connection, Robert L. Park of the APS reports that Rep. Wayne Owens (R-UT) whose district includes the U, recently sent a "Dear Colleague" letter to fellow members of Congress, urging them to keep an open mind on cold fusion in spite of the negative report of the DOE cold fusion panel.

It is expected that ONR and NSF will fund but mildly.

2.4 EPRI Meeting to discuss positive results.

Yet another meeting has been held at which results and experimenters were selected. Only positive results on cold fusion were given. Dr. Bockris of Texas A&M told me that it was a most impressive meeting. The results from Florida are the most advanced in respect of the fact that they have heat, tritium, neutrons and gamma rays.

He considers there are distinct "types of heat emission;

(1) For most of the time the electrodes do nothing
(2) For maybe a quarter of the time.....they emit a low-level heat .... 10 to 15% excess heat.
(3) What is really exciting is that from time to time the electrodes emit heat which is often 3 to 4 times and sometimes 7 to 8 times, the energy being put in. These bursts last anywhere between 10 minutes to 90 hours. Many of them last 10 to 15 hours. They stop as suddenly as they have begun."

For tritium, he says that 7 or 8 labs have it now though 2 government labs will not announce until they get reproducibility. At Texas they have tried 12 electrodes now and get tritium in large amounts from all of them, but the curious thing is that you never know when it will start - between 7 days and 10 weeks.

The argument as to whether it is a volume or a surface effect continues. Prof Bockris considers that there "are two parties now;

(a) The Fleischmann-Pons party which thinks everything is compressed inside the electrode
(b) The Bockris party which believes that things happen because of very high fields at dendritic tips on the surface."

It would be good to have some solid experimental evidence to justify these two opinions.

Hear rumours that there may be other meetings organised at which only positive results and not all results, will be presented.

3. STARTING UP OF BYU FUSION RESEARCH CENTRE

Not everyone in Utah agreed with the setting up of a National Fusion Research Institute in Utah. When Stephen Jones of Brigham Young University was interviewed for local news on channel 2, he said that he "respectfully" disagreed with the findings of the committee. "We have input that would be useful to the committee", he said. However the Y News, a paper of BYU, on 11 August wrote that "A Center for Cold Nuclear Fusion Studies
has been established on campus with B. Kent Harrison, professor of physics as director. Stan L. Albrecht BYU academic vice-President and associate provost said the centre will conduct research on a wide range of fusion-related projects, expanding on the work of Stephen E. Jones and his associates who have been conducting cold nuclear fusion research at BYU since 1986."

"Jones, an associate professor of physics and Douglas N. Bunion, a professor of chemical engineering and chairman of that department, will serve as Associate Directors of the center."

"Harrison said the centre will facilitate the scholarly exchange of information in peer-reviewed journals and at professional meetings and will correlate fusion research at BYU and with other organisations. He said the centre will also attract funding in support of research and develop commercial applications for cold nuclear fusion."

It may be seen that these two centres in Utah have quite different styles.

4. STARTING UP OF A JAPANESE INSTITUTE FOR FUSION RESEARCH
+ 5. BUILDING OF A GIANT FUSION CELL IN INDIA

Have heard some reports of a centre being set up in Japan. The fullest account is in the Salt Lake Tribune of August 24;

"The editor of an Asian science and technology journal said Wednesday that scientists in Japan have organized an Institute of Fusion Science and are rapidly moving ahead in cold fusion.
"Japan is the most organized of all the countries," said Ramtanu Maitra, editor of Fusion Asia, a journal of energy and other technology issues published in New Delhi.

Mr. Maitra was in Utah to see fusion scientists at the University of Utah and Brigham Young University. He met with BYU physicist Paul Palmer Tuesday and with U. of U. College of Mines Dean Milton Wadsworth and National Cold Fusion Institute Director, Hugo Rossi, Wednesday.

Mr. Maitra has a master's degree in nuclear physics from the State University of New York at Stonybrook, but he said he came to Utah as a scientific journalist, not a nuclear scientist. Mr. Maitra said the institute was set up Aug. 1, and some 80 scientists will join under the leadership of Hideo Ikegami, a respected scientist.

He said the Japanese are very cautious and would not embark on such a thing unless it was worth pursuing. "They have found something. It's very clear."

University of Utah officials, in their bid for fusion funding, have repeatedly raised the specter of Japanese scientists using an organized effort to commercially exploit cold fusion before the United States.

He also said the Japanese tend to take a long-term approach to their research. "They won't get very euphoric, but they won't get very discouraged either....You have to get out of the mind-set that if it doesn't happen fast, it doesn't happen at all.

"We have to remember that Japan has an advanced hot-fusion research base," Mr. Maitra said. "This is not something totally new for them."
The Japanese have been closed-mouthed about fusion, he said. It's very difficult to get information...

But he believes they will eventually be willing to publish more on fusion than their U.S. counterparts, who tend to classify such research for national security or patent reasons.

Mr. Maitra also said his own country, India, has stepped up its cold-fusion efforts at the country's nuclear research centers, including the Trombay Nuclear Research Center. Trombay, about 20 miles from Bombay, has 12,000 scientists [surely an error - it should be "staff" not "scientists"], he said.

One Indian group has build a five-foot-tall cathode for a cold-fusion cell, which far exceeds anything built in the United States. "They wouldn't have gone for it if they were't seeing anything significant," he said.
He said Asian scientists are acutely aware that cold-fusion research was launched in Utah, and several of them expressed envy that he was visiting here."

Have minimal confirmation of what Mr. Maitra has said and would welcome any completely independent confirmation. In the early days there were reports of work on cold fusion in Japan and Tokyo and Hokkaido Universities were reported as confirming some effects, but since then there have been remarkably few reports. Maybe as Sherlock Holmes said in the Hound of the Baskervilles, the real question is why the hound did NOT bark.

In India three labs have reported results which could be considered positive confirmation. Dr. P. K. Iyengar, Director of the Bhabba Atomic Research Centre at Trombay, Bombay, has sent me a paper "Cold Fusion in BARC Experiments" where he reviews a series of positive results obtained by groups at his institute. He says that full papers are being prepared and one looks forward to receiving them.

6. UTAH REPORTS NO NEUTRONS OR GAMMAS FROM PONS'S CELLS

In the previous note, No. 18, it was explained that while several of Pons's cells were running on a table in his lab, Mike Salamon, Ed Wrenn and Haven Bergeson and Collaborators were operating neutron and gamma counters underneath the same table. No fusion products were detected.

On 24 August Drs Salamon and Bergeson went public with their results which caused considerable local interest. They have written a paper for Nature. Their main result are;

a) the upper limit for neutrons corresponds to less than one billionth of a Watt from the reaction d + d ---> 3He + n
b) the upper limit for gammas corresponds to less than one billionth of a Watt from the reaction d + d ---> 4He + gamma
c) the upper limit for internal conversion electrons is about one billionth of a Watt - these would be the E2 nuclear gammas from Coulomb excitation of the even-even Pd isotopes by the 3 MeV protons produced in the reaction d + d ---> t + p

These would seem to cover the main fusion reactions.

These results would appear to exclude the helium internal conversion theory of Chaves Walling and John Simmons. And what is important today, be in contradiction with the results reported on tritium production. It would be good if it were possible for a group finding tritium were to co-operate with the Utah group to make simultaneous measurements as a single measurement of a surprising result needs confirmation by a simultaneous measurement of a different nature, to be generally acceptable.

Neither physicist was willing to dismiss Pons and Fleischmann's results but they said it would require a mechanism unknown in nuclear physics.

According to a previous agreement they showed their paper first to prof. Pons. His answer was very simple - in the six weeks in May and June when they were running, there had been no heat excursions. The Salt Lake Tribune quoted;

"'I'm not at all surprised by their results' said Dr. Pons, who said the cells they were monitoring were running at barely detectable levels. Dr. Pons said the cells had none of the heat 'bursts' that have been reported. These bursts have produced up to 50 times the energy supplied by the cells battery, Dr. Pons has reported, but they have been extremely elusive."

"'We have purposely kept the power amounts low on these cells', Dr. Pons said, explaining that he and colleague Martin Fleischann are trying to 'lower our error bars' in their heat detection."

All this is rather strange. In his previous report in No. 18, it was written "During this period we were informed at least twice that there was at least one "active" cell; during one of these times, the D2O electrolyte was personally observed to be boiling. Our discussion focusses on the episode, which lasted approximately two hours until the cell was turned off under Prof. Pons's instructions -- this was to avoid a catastrophic event.". Also Dr.Pons's group tried to generate cell activity for them by changing the current suddenly from 100 mA to 600 mA in the most promising cell (the one that boiled later). There seems a contradiction here. However it could easily be resolved by looking at the original log books kept by Pons's group. From past history, it is possible that the reply will be given that the lawyers will not allow it. This would be a surprising reply since Dr. Salamon has measured one billionth of a Watt which could be of no practical importance to a lawyer. However the argument might still be employed, so it is to be hoped that the pages will copied and stored separately and carefully to avoid any unfortunate accidental erasing of such historically important data.

The Tribune continues "Despite their null result in Dr. Pons's lab, Dr. Salamon and Dr. Bergeson are setting up a lab at the National Cold Fusion Institute. The scientists say they will not be operating any fusion cells themselves but they will be assisting others in looking for fusion particles." So far as I can make out the Institute will run almost as a funding agency with various groups being semi-independent. Thus Dr. Salamon will be a group which will be called on whenever another group finds a heat excess and his group will make independent measurements, usually of different fusion products. This seems an excellent procedure - and I hope my understanding is correct!

It strains one's belief that during the six weeks in May and June that Dr. Pons did not once wish to do a test which made a cell active, especially when he knew that his colleagues were making a test during these six weeks. However if we assume it were true, then Dr. Pons has just said "we purposely kept the

power amounts low on those cells". Now "keeping low" normally means that the power was not zero. What is the smallest possible power that Dr. Pons could measure? Certainly more than a one milliwatt (this surely will be given in their forthcoming paper). Now at a milliwatt of power from fusion, this would imply a flux of over a billion neutrons per second. Since this enormous signal was not observed, then from the results of Dr Salamon et al. and the statement of Dr. Pons, it must be concluded that the heat they observe cannot originate from nuclear fusion".

7 DISCUSSSION OF POSITIVE RESULTS REPORTED.

7.1 Florida results contested. Upstairs/Downstairs.

In May I heard that at the University of Florida in Gainsville, evidence had been found for tritium in cells. However I then heard that there were two groups working in the Nuclear Science building.

It was the Downstairs group of G. Schoessow and J. A. Wethington, both Emeritus professors of Nuclear Engineering, who had held a press conference to announce the observation of tritium in their one cell which had been running for a few days.

The Upstairs group consisted of 9 people, who are a mixed team of nuclear chemists, electrochemists, condensed matter physicists and nuclear physicists. They had been running much longer and studying calorimetry, neutrons. gammas and tritium. They did not find any effect. They also pointed out that the Downstairs group were working with heavy water that was heavily contaminated with tritium.

Felt that this affair was closed so commented very little about it.

However on 4 August on the KUER radio fusion update programme of NPR, Prof. Schoessow said that they claimed to see tritium. In the CNF summary part 1, version 2 is written "After 48 electrolysis, they find about 1 E9 tritons. After 100 hours, they find about 2 E10 tritons. A control run without current produced negligible tritium. They subjected the the Pd to a 'special treatment' before the experiment but are uncertain which 'adaptation may have contributed to
their findings.'"

" On the radio he said that he has a cold fusion cell of his own design, patent pending. He claims he can vary the heat output continuously, and on demand, from no excess heat to 200% of the input energy. He has measured tritium at a rate of 50 000 disintegrations per minute per ml of electrolyte. He said he has deliberately not said much about his work. He did not want it to leak out before he was sure of his results. His university had asked him not to speak to the press, but he had granted an interview to NPR before the request was made. He went on to say he had been called by many companies that are trying to get a cold fusion cell working. He said they often have 20 or more cells running and cannot get any effect.

Prof. Bockris has been reporting to me that the observations of tritium at Florida confirm their own work in Texas A&M. He feels they are the most advanced series as they have heat, tritium, neutrons and gamma rays. He says he is most impressed by them especially as they are respectively 81 and 75 years old (though still younger than Lineus Pauling who had a paper on Cold Fusion published in Nature at the age of 88 years). The results of the Downstairs group have apparently made a great impression in Utah and greatly helped the new National Cold Fusion Institute. Prof. Bockris says he is going to visit them these days.

I have sent a message to them asking for copies of their results as it is not possible to judge from the indirect messages I have received.

The Upstairs group have continued their measurements with one to three cells. They find no evidence for cold fusion and therefore do not endorse the claims of Drs. Schoessow and Wethington. In particular their methods are different.

The situation seems unclear but since they are neighbours it should be possible to arrange for an internal workshop to discuss exhaustively the two sets of experiments. Naturally I am sure that in these circumstances it

would be best for the reputation of the Department, if there were no publications, press releases or TV interviews.

PS This evening, 30 August, received a message that the Upstairs group leader, Dr. Muga and Dr Achey have talked with professors Schoessow and Wethington who said that they only claimed excess heat and they did not claim particles or tritium. When asked specifically about tritium, Prof. Schoessow replied "We do not want to see tritium".

7.2 The Texas A&M Tritium Results

Prof. Bockris sent me the preprint of their paper "Production of Tritium from D2O Electrolysis at a Palladium Cathode" by N. J. C Packham et al. which they had submitted to Nature but the referees reports were unsympathetic. He asked for my opinion. The paper reports on 24 cells tried. Results of measurements for tritium for nine of them are tabulated for a variety of running conditions and activities up to 5 E7 disintegrations per minute per ml were given. These are very high rates and my immediate concern was for the health of the experimenters. If the tritium was from the reaction

d + d ---> t + p

then the 3 MeV protons would give large numbers of neutrons from the charge exchange reaction, p + n ---> n + p , and the 1 MeV tritons would produce neutrons from t + d ---> n + 4He. A serious biohazard.

There were checks that the initial materials did not contain tritium, but the obvious check that has been many times discussed (e.g. Nature) to repeat the measurements with ordinary water, H2O, had not been done. Also the check where the cathode was replaced by an inert material such as platinium had not been done. As a fairly experienced referee and editor, this was sufficient grounds for asking for further work as when one claims an important result, normal standards of refereeing should apply. In addition the paper was not up to normal standards in several other respects.

7.3 General Electric

It was written in a confidential report that was obtained by the SL Tribune, that GE personel had found 15% excess heat. However in a radio interview, Hugo Rossi, the new Director of the National Cold Fusion Institute explained that he had written the letter himself to the Council. He said that the GE scientists had been given three P&F type cells prepared in Pons's own lab, one of which (according to P&F's measurements) consistently produced 15% excess heat. The three cells were taken back to Schenectady for GE scientists to make their own measurements. There was evidently some question in the minds of the GE people over the calibration and Rossi stated that if a conservative view was taken, the 15% would evaporate. Rossi made it clear that GE scientists had not confirmed excess heat.

7.4 Other Positive Results

7.4.1,2,3 The three positive results that most impressed people at the EPRI meeting were from Florida, Texas and GE and they have been discussed above. About the other quoted results;

7.4.4 Utah - see measurements of Salamon et al. discussed earlier.

7.4.5 Drs Storms and Talcott at Los Alamos National Lab. The Lab has called these results preliminary. It is said that after the first two cells subsequent cells (60 of them!) had failed to show an effect.

7.4.6 Dr. Scott of Oak Ridge National Lab - the Lab spokesman called these results "preliminary"

7.4.7 Dr. Mackubre of Stanford Research Institute has claimed 8% excess heat. The EPRI funding agency described them as "some indications of heat, but they have been sporadic". More information is necesary.

7.4.8 At Case Western Reserve University, "low levels of excess energy" have been reported - more information is necessary.

7.4.9 Tritium production at the University of Colorado at levels well below

that corresponding to claims of excess heat - again more information is necesary to judge

7.4.10 Prof. Huggins of Stanford University has reported 12% excess heat based on observing that the cathode got warmer when D2O was used than when H2O was used - however this is natural given the different properties (e.g. heat capacities) of the two systems - but await more information, including these effects being taken into account.

7.5 Summary of Positive and Negative Results

There are a number of experiments that have given positive results, but if one asks that the experiments have all the checks done (including H2O and dummy cathodes), be well analysed with conventional statistics and show a statistical significant effect consistently, then I do not know of one which satisfies these normal scientific requirements.

On the other hand there are groups which have done careful work, explained it, obtained consistent results, but these groups all obtain null results i.e. they observe no cold fusion effects. Examples are Harwell, Karlsruhe, Yale-BNL, ATT, Bugey-Frejus, Caltech and several others.

Many of the groups obtaining positive results emphasise that the effects they observe are sporadic and irregular. They cannot predict when they happen. Normally this is taken as a sign that the results are untrustworthy. But this is turned into an advantage saying it shows this is something new and needs further investigation. But before further investigation it is necessary to establish that any effect is taking place at all and this means doing an experiment that meets with the criteria of normal science. If one wishes to believe the effect is caused by fusion then a reasonable experiment would try to observe more than one fusion product simultaneously and do the elementary check that the rates deduced from each fusion product are in agreement.

If only there were one good experiment that was well done and well described. Just one.

8. OTHER ITEMS

8.1 Visit to Harwell

Visited Harwell and spent some time with David Williams. Discussed the Harwell work - it is very impressive and extensive. They have written a major paper for Nature, but when I checked with David today, they still had not heard whether it has been accepted. But the essential results are they they do not see any excess heat, neutrons, gammas, tritium or helium and have been able to give upper limits on processes which are very low.

8.2 Report from Berlin

Prof Marx of the Freie University of Berlin (who had worked with Prof. Kreysa of Frankfurt) told me that they had put tritium also in their cell and found that they quickly had DTO. They looked for neutrons and found "nothing, absolutely nothing"

8.3 Comments on DOE Panel report by Dr Pons and Senator Garn

The Deseret News printed comments on the interim report of the Department of Energy Panel by Dr. Pons;

"'It's a totally useless committee that is telling untruths. It serves no purpose.' He said he agreed with a statement made by Senator Jake Garn, R-Utah.

Garn critised what he sees as dirty politics in the scientific community for smearing cold fusion research and ruining its chances for large federal spending.

'I used to think politicians were dirty, slimy dishonest people. I've decided that the scientists are far worse than politicians', Garn said.

'I wholeheartedly agree with him' Pons said. 'In fact I am thinking about changing professions and becoming a politician'".

[and I used to think US Senators were distinguished responsible people]
Actually Dr. Pons had started attacking the Panel in the Salt Lake Tribune the day before, agreeing with Dr. Bockris that it was a "killer commission" and attacking Nature editor John Maddox especially because of his editorial calling on scientists 'to dismiss cold fusion as an illusion'. "'John Maddox's problem is he only reads his own newspaper", Dr. Maddox said, emphasising that Nature 'is a newspaper not a scientific journal.'"
[Pity, I've just ordered Nature and rather enjoy it].

8.4 Paper from Madrid

In June was invited to go to Madrid where the group of Carlos Sanchez had detected neutrons with a BF3 detector plus some gammas and variations in tritium concentration. They have now sent me a copy of their paper which will be published in Solid State Comm. The paper gives the data and at the foot of the table is written "After finishing this record (on 9 June) the current was switched on and the experiment is still running in the same conditions. From that day until now only background-like signals have been recorded from detectors and counters." [Carlos is a very kind man and I was most impressed by a physics course that he has set up for blind students, which particularly interested me. There are also practical demonstrations that he has built. In Note No. 15, I asked if any other university had a similar course for blind students. So far I have had no replies - does this mean that Carlos's course is the first?]

8.5 Changes at Los Alamos

In the subject of cold fusion, Los Alamos has tried to be very fair and even-handed. However at the Santa Fe conference that they mainly organised with varible success, people from Los Alamos presented results which both established cold fusion and showed that it did not occur. The null results have not, as far as I know, been questioned, but the reports of positive results have been controversial. At the meeting, after the evidence for neutron bursts had been presented, I asked if the essential control experiment of using normal H2O had been done and was told by Dr. Menlove that it had not been done but they would do it next. However they have now issued a preprint in which other experiments have been added but this promised check is not reported. Have asked their BYU collaborator several times about this but so far have not had a reply. Now I hear from various sources that they have trouble repeating these sporadic bursts - it would be good to have this cleared up. There is also considerable confusion over the press reports that Drs Storms and Talcott had found tritium but the Lab called the results preliminary. Again they may be having trouble repeating their results. The Wall Street Journal of 19 July carried an advertisement for a Director and Deputy Director of Public Affairs.
[It is often difficult to be fair and balanced. For example if you were making a film where the earth is shown to be round, would you agree to a demand from a member of theFlat Earth Society for equal space to show the earth was flat?]

8.6 European Physical Society

It is interesting to see how opinion among physicists changes with time about cold fusion. In May the American Physical Society organised two sessions on cold fusion but for their bi-annual meeting next month the European Physical Society has decided that cold fusion should not be discussed.

8.7 High Altitude Cold Fusion

In the August 21 edition of Community College Week, it is reported that at Colorado Mountain College they are trying cold fusion in their science lab which is at 10 000 feet. This is part of a large project by Rockwell International to test several variations of the Utah experiment. Associate professor Peter Jeschofnig and student volunteers have monitored the cold fusion experiment using equipment supplied by Rockwell. The

company collected the data files as well as gases generated by the experiment to conduct further research. Dr. Jeschofnig said "the college altitude test did appear to generate fusion. It looks like a positive test" he added. Rockwell received funding for its test through the Department of Energy. At other sites the company is using several other variations in addition to altitude.

The aim of the study at high altitude was to look for muon-generated fusion. It is possible that it was not known that more decisive and better controlled experiments have been performed and reported on of tests on cold fusion using muon beams. Also theoretically one would expect the muons to be quickly trapped on the high-Z palladium cathode.

Douglas R. O. Morrison

PS There is no truth in the rumour that the five-foot cathode is really intended for Kali.

PPS Some scientists in Utah have begun to refer to the inconsistencies coming out of the Cold Fusion establishment as HEAVYWATERGATE.

NOTE ON LEP

During the five or so days of the pilot run of LEP, some collisions were obtained during less than 16 hours and the four experiments found a total of over 50 examples of Z0 events. Commissioning of LEP is continuing and a beam of over one milliAmp has been obtained. It is planned to start a physics run on 11 September.

U.S. DEPARTMENT OF COMMERCE
NATIONAL INSTITUTE OF STANDARDS AND TECHNOLOGY
(FORMERLY NATIONAL BUREAU OF STANDARDS)
BLDG 245 ROOM B109
GAITHERSBURG, MD 20899

UNITED STATES DEPARTMENT OF COMMERCE
National Institute of Standards and Technology
(formerly National Bureau of Standards)
Gaithersburg, Maryland 20899

July 24, 1989

Richard L. Garwin
IBM Fellow & Science Advisor
to the Director of Research
IBM Corporation
Yorktown Heights, NY 10598

Dear Professor Garwin:

Enclosed please find the preprint "Coulomb assisted cold fusion in solids." This paper describes a mechanism which indicates that actual cold fusion may be compatible with the usual quantum mechanics and does not require outlandish assumption. I hope you will find this paper interesting, and I would be very happy to hear of any comments you may have.

Sincerely yours,

Michael Danos
Center for Radiation Research
Nuclear Physics Group

Enclosure

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(3.4)

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

August 11, 1989

Dr. Michael Danos
Center for Radiation Research
Nuclear Physics Group
U.S. Department of Commerce
National Institute of Standards
and Technology
Bldg 245, Room B109
Gaithersburg, MD 20899

Dear Dr. Danos:

Thank you for your note of 07/24/89 and the enclosed preprint of 07/18/89, "Coulomb Assisted Cold Fusion in Solids." While I understand that a calculation for comparison with experiment might need to take into account "very subtle characteristics of solids," I would still like to see a model calculation which would at least show me one example of a calculated rate. I would like to see numbers.

For instance, the model could be two deuterium atoms in a small box with a single (modelled) high-Z atom.

Thank you.

Sincerely yours,

Richard L. Garwin

RLG:jtml:223%MD:081189..MD

UNITED STATES DEPARTMENT OF COMMERCE
National Institute of Standards and Technology
(formerly National Bureau of Standards)
Gaithersburg, Maryland 20899

(3,5)

August 21, 1989

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Garwin:

As you note in your letter of August 11, 1989, my preprint on "Cold Fusion" of 07/18/89 concerns itself only with pointing towards the existence of previously ignored effects which, when taken into account, have the potential of changing the expected magnitude of the cold fusion rate by many orders of magnitude. However, no numbers were reported in that paper. Let me then begin by supplying that information, while below I shall expand a little on the preprint, pointing toward the next steps. The point of departure from the "standard" treatment is eq. (9) of the 07/18/89 preprint, in that it allows $B \neq 0$, i.e. the presence of the irregular Coulomb function in the solution. Thus, the "standard" solution is reproduced if ~~our~~ one drops in (23) and (24) the first term in the brackets. Going through the algebra one finds that this results in achieving for a^2, eq. (25), the off-the-resonance value, which contains the factor e^{-2X}. This factor is the "standard" Coulomb penetration factor for which depending on the model, values 10^{-20} to 10^{-50} have been reported. Thus the numerical result of the "naive" rate, i.e. the rate computed with eq. (41) assuming a fully thermalized system, turn out to be in the range 10^{-10} to 10^{-20} sec^{-1}/(fusing pair). This value I reported at the NATO Summer School, May 1989, "The Nuclear Equation of State." I enclose the write-up of that lecture.

However, as explained to some length in the 07/18/89 preprint, a non-thermalized state would yield zero fusion rate. Hence, the above numbers represent limiting fusion rates. In fact, this dependence on the character of the density matrix to be thermalized may be - in terms of a wild speculation - suspected to be the reason for the observed irreproducibility of the fusion rate (cf. the Texas A&M tritium production), and the erratic behavior even after tritium production has commenced. Here the speculation:

The deuterium in the Pd lattice may be in a coherent BCS-type state. Still, since the d is only a pseudo-Boson the coherent state actually must be split, i.e. must be a very dense spectrum of $\sim 10^{23}$ states; level spacing $\sim 10^{-26}$ eV (assuming, a total width ~1 meV). Since the width Γ, eq. (27) is $\sim 10^{-40}$ it is very much smaller than the level spacing; there may not be any overlap between the line, eq. (25), and the available states. While the electrolysis is taking place, this coherent deuterium state is brought into contact with an external heat bath, by means of the incoherently entering deuterons; rate $\sim 10^{19}$/sec. This thus may be acting as a coherence destroying input. After 2 weeks some 10^{25} such random de-coherenting events will have happened. This may suffice to allow for

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"naive" fusion to take place. In this (speculative) mechanism one may "understand" the role of the electrolysis. Also, switching off the current would interrupt the thermalization; as the resonating states, which can undergo fusion, react the potential fusion population disappears and the reaction stops. Of course, this speculation is too wild for publication.

I only want to mention that other processes, in addition to the lowest order process of the 07/18/89 preprint, are possible. For example, the next higher order photo-disintegration process of Fig. 1 has the

1

2

3

potential of yielding a very large $(p + t)/(^3H + n)$ branching ratio since in the former the neutron is absorbed by the deuteron 3, while in the latter it is the proton (which experiences Coulomb repulsion) which must be absorbed by 3.

In closing I want to mention that to my best knowledge only one other physicist, namely V. Belyaev, Dubna, has reported similar thoughts.

Sincerely yours,

Michael Danos
Center for Radiation Research
Nuclear Physics Group

COULOMB ASSISTED COLD FUSION

Michael Danos

Center for Radiation Research
National Institute of Standards and Technology
Gaithersburg, MD 20899

INTRODUCTION

By now several experiments have been reported which suggest that light-element fusion occurs in solids having the capability of carrying hydrogen. The present lecture contains a formulation for the computation of the fusion rates, and an estimate of the order of magnitude of the rates which could be expected in semi-heavy water (HDO) and in hydrides.

Consider that a hydrogen, denoted by m_2 (say, a proton), is contained in a trap, next to a lattice nucleus, M, denoted by m_1. If a second hydrogen, m_3 (say, a deuteron), drifts past this site, as it reaches a position such that m_1, m_2, and m_3 are on a line with the Coulomb repulsion can impart a momentum to m_2 "propelling" it towards m_3 and at the same time transferring the recoil $\vec{q}$ and the energy E to m_1, keeping it on the mass shell. Hence m_2 now is off the mass shell. As m_2 reaches m_3, fusion takes place yielding the nucleus m_4 (here ^{3}He), and all particles are restored to be on the mass shell. The momentum space Feynman graph is shown in Fig. 1. Writing ($\hbar = c = 1$)

$$N(t) \sim \psi_0(t) \frac{1}{t_1^2 - m_1^2} \frac{1}{t_2^2 - m_2^2} \tag{1}$$

$$M \sim e^2 F(q) f(q) \psi(p) \frac{1}{q^2} \frac{1}{p^2 - m_2^2} \tag{2}$$

the matrix element of the graph is

$$R \simeq M \int dt\, N(t) \quad . \tag{3}$$

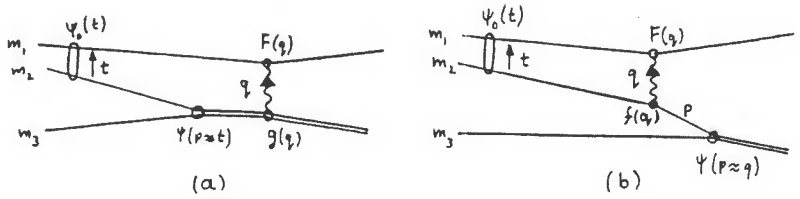

Fig. 1. Feynman graphs for Coulomb assisted fusion. m_1: lattice nucleus; m_2,m_3: fusing nuclei.

In (1) $\psi_0(t)$ is the trapping wave functions; very little is known about it except that the momenta $|t|$ are "small," appropriate to lattice physics, i.e., of the order d^{-1} where d is the lattice spacing. $F(q)$ is the Coulomb vertex function of m_1, i.e., of the nucleus M, $f(q)$ that of m_2 (the proton), and $\psi(p)$ is the nuclear fusion vertex function. Of these quantities $F(q)$ is known from electron scattering; $f(q)$ and $\psi(q)$, being half off-the-mass-shell, must be estimated from on-the-mass-shell experiments.

Even though here a time-independent treatment most probably is not practicable, it nevertheless is useful to take a look at the qualitative features of such a description as an aid in achieving an understanding of the process. Thus, being a stationary state description, the system would have standing waves in all channels. In other words, the time-independent treatment is a superposition of the reaction and its time-reverse. In order to give an illustration of the process, and to approach the description of the graphs of Fig. 1, we replace the full-fledged three-body Coulomb problem by a two-stage description, in the first stage dropping the Coulomb interaction between m_1 and the other particles, and regarding its effect in the second stage. Before fusion we have a three-body system, after the fusion a two-body system. This final state system is kinematically fully determined. Thus, taking A_1 to be ~ 100, we have the following kinematic conditions for the recoil to the lattice nucleus: for the p+d $\rightarrow$ ^{3}He reaction: $p_1' \approx 1$ fm^{-1}, $E_1' \approx 0.16$ MeV; for d+d $\rightarrow$ ^{4}He: $p_1' \approx 2$ fm^{-1}, $E_1' \approx 0.95$ MeV. Thus $t/p_1' \sim 10^{-5}$ and can be neglected. The energetics of the reaction is schematically illustrated in Fig. 2, where the (9-dimensional) configuration space is symbolically split into region I containing the 3-body system and region II containing the 2-body system. (Region I excludes the space $r_{dn} \lesssim a \approx 5$ fm where nuclear forces become important). Three of the infinite number of configurations are illustrated. Figure 2a shows the distribution of energies corresponding to the initial state of Fig. 1. Figure 2b shows the energy distribution of the configuration corresponding to the intermediate state of Fig. 1b, i.e., the state of the system after exchange of the "Coulomb photon" q, but before fusion. Thus particle 1, the Pd nucleus, has its final energy and momentum while 2 carries the momentum of the final state of the fused (^{3}He) system. Particle 3 is still in its initial state. Figure 2c shows another of the possible configurations, where particles 1 and 3 have undergone a further energy-momentum exchange. This state thus requires the exchange of 2 "Coulomb photons" for it to be reached from the "initial" configuration, Fig. 1a.

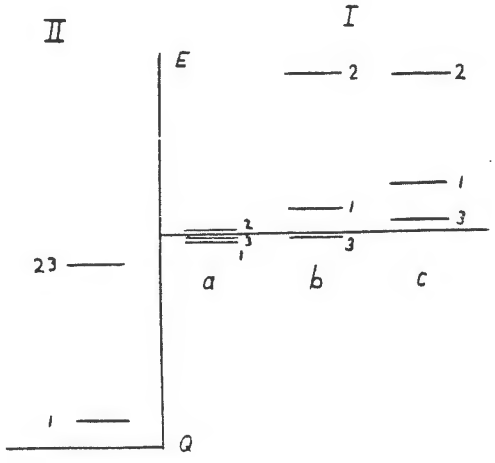

Fig. 2. Energies for different configurations: a. initial state; b. final state; c. some other configuration.

Region I: before fusion;
Region II: after fusion.

We now look at the qualitative behavior of the relative wave function of the m_2-m_3 subsystem. Consider first the configuration Fig. 2a. There the relative wave function is essentially exponentially damped owing to the Coulomb barrier (Fig. 4a). This wave function usually is computed by the WKB method. However, in contrast to the WKB wave function, at $r_{23} \sim a$ the solution requires the presence of the irregular Coulomb solution in order to allow the fulfilling of the matching conditions at the nuclear surface $r_{23} = a$. This admixture can for certain reconance conditions change the barrier penetrability by many orders of magnitude from the WKB value.

On the other hand, the wave function which corresponds to the configuration of Fig. 2b, being above the Coulomb barrier, remains oscillatory down to $r_{23} \approx a$. A very small admixture resulting from the action of V_{12}, V_{13}, will thus "circumvent" the influence of the Coulomb barrier. This is precisely the content of the graph Fig. 1b. In the time-independent Schrödinger picture the admixture of this configuration thus will be proportional to

$$A \sim \frac{\langle a|(V_{12} + V_{13})|b\rangle\langle b|(V_{12} + V_{13})|a\rangle}{\varepsilon_b - E}$$

which may be a small, but not "exponentially" small, number.

In order to estimate the fusion rate we must address the characteristics of the reaction. We observe that the wavelengths of the recoiling particles m_1 and m_2 are $\sim q^{-1} \sim 1$ fm, while the size, d, of the system is $\sim$ Å. Hence the motion of the particles is properly described by geometric (ray) optics rather than by wave optics. (Formally, this is related to the angular parts of the Fourier transformation.) This is the basis of the observation that m_1, m_2, and m_3 must be aligned. Only then can the Coulomb force, which has the direction given by $\vec{r}_2 - \vec{r}_1$, impart a momentum $\vec{q}$ (which thus also has the direction $\vec{r}_2 - \vec{r}_1$) which can carry m_2 towards m_3 resulting in fusion; m_3 must lie in the "shadow" of m_2. For uncorrelated positions this leads to a reduction factor

$$\eta_0 \sim (qd)^{-2} \approx 10^{-8}$$

Owing to the presence of correlations the actual reduction factor for fusion in such crystals thus can be very much smaller than η_0. For fusion to occur, therefore a special class of crystal imperfections is needed which would provide traps for hydrogen such that fusion alignment might occur. Simply using geometrical reasoning one finds that η in some cases could be as large as 10^{-4}. On the other hand, for example in HDO the alignment will be suppressed even further; here η may be as small as 10^{-14}.

Turning now to (3) we note that if the propagators in (1) were absent, the integral in (3) would yield $\psi(r_{12} = 0)$, i.e., it would essentially vanish. This, of course, is not the case. However one sees that the actual result depends rather sensitively on the (unknown) details of $\psi_0(t)$ in a rather involved manner since the propogators in position space are integral operators.

To estimate the reaction rates note that the central mechanism of Fig. 1 is the (off-the-mass-shell) Coulomb scattering of m_2 on the much heavier m_1. The other parts of the graph yield factors which decrease the reaction rate by differing amounts. Dropping numerical factors like P we estimate for the case of pd fusion in a palladium lattice: $(F(q))^2 \sim 10^{-2}$, $(f(q))^2 \sim 1$, $[\psi(p)/(p^2-m^2)] \approx 10^{-2}$, $[\int dt\ N(\,)t)]^2 \sim 10^{-5} \times 10^{\pm 3}$, and $\rho v \sim 10^{-18}\ fm^{-3}$. With the above estimate for the angular factor 10^{-4} we obtain

$$\text{Rate(pd, lattice)} \simeq 10^{-12} \times 10^{\pm 3}\ (\text{trap sec})^{-1} \quad .$$

For dd fusion we estimate

$$(F(q))^2 \sim 10^{-4},\ (f(q))^2 \sim 10^{-3},\ \text{and}\ [\psi(p)/(p^2-m^2)]^2 \sim 10^{-4}.$$

Thus

$$\text{Rate(dd, lattice)} \sim 10^{-17} \times 10^{\pm 3}\ (\text{trap sec})^{-1} \quad .$$

To conclude, we give an estimate for heavy water. The main difference resides in the above mentioned angular suppression factor. Hence the HDO fusion rate is estimated to be around $10^{-24} \times 10^{\pm 3}$ (molecule sec)$^{-1}$, while in D_2O the rate may be a factor 10^5 smaller.

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

September 1, 1989

To: Cold Fusion Panel

The following items are circulated for your information.

- Bockris/Bigeleisen Correspondence
- Rossi/Huizenga Correspondence

William L. Woodard

Enclosures

TEXAS A&M UNIVERSITY

DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

August 7, 1989

(409) 845-2011
FAX (409) 845-4719

Dr. Jacob Bigeleisen
State University of NY at Stony Brook
Chemistry Department
Stony Brook, NY 11794-3400

Dear Professor Bigeleisen:

I'm writing again on the question of our tritium measurements, which have continued.

In an article in one of the journals, you are quoted as saying that you thought the tritium measurements we have must be due to contamination.

This has been suggested in respect to the work of other people who have gotten tritium, too.

It was also suggested by the referees for a paper which I sent to Nature and which was rejected on the grounds that the referees were sure the measurements must be due to contamination.

We have rewritten the paper concerned, and we've also done a good deal more work on contamination since we first sent the paper to you. Specifically, we have examined the possibility of existence of tritium in all parts of the apparatus, including palladium and nickel electrodes, the glass, etc., apart from more obvious sources such as the deuterium oxide.

All these measurements are now recorded in the paper, and I'd ask you to have a look at it and to give me your opinion as to whether you think that, on the new evidence given, there is any possibility of contamination being the origin of these tritium measurements.

Another very important thing in respect to the tritium is that it is observed widely. Thus, tritium has been measured by Ramirez at the Institute of Petroleum in Mexico City (a paper in press), by Storms at Los Alamos, by a group at Oak Ridge, and by Schoessaw and Wellington (two nuclear engineers working at the University of Florida in Gainesville).

The work in the government labs (It is also being observed at Oak Ridge.) won't be admitted because of the implications.* However, I have talked to the people concerned, and I'm convinced that they are extremely competent and experienced professionals and that their measurements are real.

* And because reproducibility is admittedly poor.

the interior of the palladium.

At the electrode-solution interface, there is a very strong electric field, and in planes in contact with the solution this amounts to an amount of the order of 10^8V/cm.

At points and spikes which certainly grow in such electrodes, it is very much more.

At the transfer of a deuteron from its solvation sheath with deuterium oxide some 5Å out into the electrical double layer formed at interfaces to land on an adsorbed tritium, there is a strong vectorial influence on the deuteron, and it would orient itself in a certain direction so that the eventual collision between the two deuterium particles would occur not in a random sense in respect to their directionality, but in a specific sense owing to the orientation of the deuterium oxide in the solution.

I think this may be the clue to the fact that we obtain such a small component of the neutron part of the branching chain. The deuteron would be oriented towards the electrode, and this would give an enhanced chance of a proton transfer reaction.

Of course, this is a speculation, but there are calculations backing it which work out quite well in order of magnitude terms.

I would hope you will be able to react to these statements, particularly to whether you think the material now in the paper is sufficiently evidenced in respect to the absence of contamination of these measurements.

Sincerely,

J. O'M. Bockris

JOMB/nm

StonyBrook

Department of Chemistry
State University of New York at Stony Brook
Stony Brook, New York 11794-3400
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26 August 1989

Professor John O'M. Bockris,
Department of Chemistry,
Texas A & M University,
College Station, Texas77843-3255

DEar Professor Bockris,

I thank you for the courtesy of sending me a preprint of your revised manuscipt "Production of Tritium from D_2O Electrolysis ata Palladium Cathode". I beleieve your reference to Table 3b on the top of page 7 must be a typographical error.

The discussion of the results of cells A2 and A7 suggests that the tritium may be produced in bursts. You will recall that when Mr. Packham showed the ERAB Panel Fig. 3 of the present manuscript at the time of our visit to Texas A & M on 19 June 1989 I questioned the uniqueness of the curve drawn through the 4 points. I suggested that the data were equally consistent with a horizontal line showing zero tritium up to 4 hours of cell operation and then an abrupt increase in tritium. **At that point Professor Kevin Wolf asked "are you suggesting that someone spiked the sample".** I said something to the following effect: "no, that was your (Wolf's) suggestion. I was inquiring as to whether the tritium, like the neutrons (and phonons) appear in bursts". The only statements I have made with respect to the question of contamination as the source of the tritium in your experiments quote Professor Wolf and my response.

I would like to get the reference to the journal article which quotes me without authorization.

I am interested in your results of the specific activity of the tritium in the off gas reported in your present manuscript. Cell A8 has a tritium specific activity of water formed from the D_2-DT electrolysis product of 5×10^7 dpm/ml. On the other hand the liquid from the cell has a specific activity of 5×10^5 dpm/ml. Your presentation of this result in the text,

item 4 on the bottom of page 9, needs revision. Certainly, the fractionation factor $(DT/D_2)_g$ vs. $(DT/D_2)_{dissolved}$ is not 100. The Henry's Law constant for D_2 in D_2O may be 100. Now I want to know, in your view does the activity in the activity in the liquid correspond to dissolved DT or is it DTO? If the latter, is it formed by slow exchange between the gas as it is generated at the cathode with the liquid?

In connection with your own work on tritium in the electrolysis of D_2O and the work by Storms, which you quote, I call your attention to the paper by A. Farkas, Trans. Farad. Soc. 33, 552-9 (1937), "The Electrolytic Separation of the Hydrogen Isotopes on a Palladium Cathode". Although this work was done over 50 years ago and used microthermal conductivity cells for measurement of D/H ratios, all the work by A. Farkas and also by L. Farkas has stood the test of time. In particular the work cited above was done in Rideal's Laboratory with support from ICI. How can it be wrong with such credentials?

We are now five months after Fleischmann and Pons announcement. The question of neutron production, the most sensitive test for cold fusion, is settled. Questions of neutron production at rates less than 10^{-5} $second^{-1}$ or bursts of 100 neutrons have no practical importance with respect to energy production. Most of the questions with respect to excess heat (power) production are also now well clarified. The Interim Report of the ERAB Panel calls attention to the need for additional experiments relevant to tritium production. In all of its work the ERAB Panel reqiuires documented claims; we cannot go on word of mouth reports or rumors. We find that statements attributed to scientists doing the work are in some cases denied by the scientists doing the work. This appears to be the case for a number of the claims of tritium production you cite in your letter to me of 1 August 1989.

Sincerely,

Jacob Bigeleisen

Jacob Bigeleisen
Distinguished Professor Emeritus

xc: ERAB Panel

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390 Wakara Way
Salt Lake City, UT 84108-1214
Telephone: 801-581-5571
FAX: 801-581-6674

August 23, 1989

Professor John Huizenga
Professor of Nuclear Chemistry
Department of Chemistry
University of Rochester
Rochester, NY 14627

Dear Professor Huizenga,

I am writing to you as the Director of the Solid State Fusion Research Project at the University of Utah. Both Drs. Pons and Wadsworth have notified me of the Panel's request for further data generated by their putative cold fusion experiments. I have to express concern on behalf of the scientists involved on the proposition presented to us by this request. Our experience with the Panel has been that preliminary data has not been treated as such, and subsequent attempts to correct misinterpretations to which preliminary data are easily subject has been viewed as rationalization. We would most certainly not encourage a repetition of that experience. Let me ask these questions, the answers to which will greatly enhance our cooperation.

1. Will the responses be treated as confidential?

2. If so, will the data be treated as preliminary? Operationally that means this: should questions arise, will they be directed toward the researchers, and will their responses be incorporated as part of the primary data?

3. If these responses are not confidential, will they then be open, in the sense that all correspondents will be informed of all the questions posed to all researchers, and all the responses distributed to all the respondees?

Continued of Page 2

I hope that I can assure you hat these questions are posed in a spirit of full cooperation, and I hope that the answers will be such as to promote the fullest possible exchange of information.

Sincerely,

Hugo Rossi

Hugo Rossi, Director
Cold Fusion Research Project

HR:rgt

UNIVERSITY OF
ROCHESTER

COLLEGE OF ARTS AND SCIENCE
DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

August 29, 1989

Professor Hugo Rossi, Director
Cold Fusion Research Project
University of Utah
390 Wakara Way
Salt Lake City, Utah 84108-1214

Dear Professor Rossi:

Thank you for your letter of August 23, 1989. Our Panel is, of course, very interested in obtaining an up-to-date summary of results from the laboratories of Professors Pons and Wadsworth. In particular, we are interested in a complete set of data with H_2O, calibration curves at different power levels and temperatures as well as recent data on neutrons, tritium and helium from working cells producing excess heat.

Your statement that "Our experience with the Panel has been that preliminary data has not been treated as such, and subsequent attempts to correct misinterpretations to which preliminary data are easily subject has been viewed as rationalization" comes as a surprise to me. In my experience the Panel has acted responsibly in executing its charge. The only misunderstanding that I am aware of has been the exchange about whether the cell in Professor Wadsworth's laboratory was operated at constant current or constant voltage. Possibly you could be more explicit in your reference to the Panel's treatment of preliminary data. At this stage of our work, we are not requesting preliminary data but unpublished results that have been checked and are at the stage where the authors believe the results and are ready to prepare them for publication. If Panel members have questions about such data that we have received, these members will certainly contact the researchers directly.

With regard to confidentiality, you must know that our DOE Panel has open meetings and the press and interested individuals may attend. Hence, within the open-meeting format, we cannot guarantee that all responses will be confidential. On the other hand, in answer to your third question, individual responses will not be open as you state, "in the sense that all correspondents will be informed of all the questions posed to all researches, and all the responses distributed to all the respondees".

In writing its final report the Panel is requesting summaries of results from a large number of workers in the field of cold fusion research representing the whole spectrum of the research community. We sincerely hope that you will send us your data so that we can include it with all the other data available to us for consideration during the Panel's working sessions in September and October.

Sincerely,

/s/
John R. Huizenga

467 Hutchison Hall
University of Rochester
Rochester, New York 14627
(716) 275-4217

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

September 1, 1989

To: Cold Fusion Working Group Coordinators (Messrs. Bard, Birnbaum, Huizenga, Fowler and Schiffer)

Subject: Jacob Bigeleisen's Attached Memorandum

Dr. Bigeleisen, as you can see from the attached, will be at Oak Ridge September 11-12. In the interest of saving time, please relay any questions directly to Dr. Bigeleisen.

Bill
William L. Woodard

Enclosures

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State University of New York at Stony Brook
Stony Brook, New York 11794-3400
(516)-632-7905
BITNET: JBIGELEI@SBCOMAIL
FAX: 516-632-7960

StonyBrook

MEMORANDUM

Date: 28 August 1989

To: J.R. Huizenga

From: Jacob Bigeleisen JB

Subject: Visit to ORNL

I will be making one of my regular visits to Oak Ridge on 11-12 September. I have been invited to meet with the people doing the work on "cold fusion" and give a seminar talk on the "Findings and Recommendations of the ERAB Panel". Could Dave Goodwin or Bill Woodard contact the ERAB Panel members responsible for the various sections of the Final Report and have them relay to me any questions about the Oak Ridge work. In addition, perhaps they could bring to my attention any questions, which the Oak Ridge people may be in a position to clarify.

xc: W. Woodard

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Stony Brook, New York 11794-3400
(516)-632-7905
BITNET: JBIGELEI@SBCCMAIL
FAX: 516-632-7960

StonyBrook

MEMORANDUM

Date: 26 August 1989

To: File

From: Jacob Bigeleisen JB

Subject: Variability of Tritium in D_2O - Samples with the same Lot Number

This is a report of a small investigation I have made about claims by Wurden et. al.[1] that different bottles of D_2O carrying the same lot number and purchased from Merck, Sharpe and Dohme can differ by orders of magnitude in their tritium assay. I called Merck, Sharpe & Dohme and spoke with the person responsible for their D_2O distribution program. They currently purchase drums of D_2O, presumably from Ontario-Hydro, filter it and distill it. All samples from the same drum and processed in a given batch are given the same lot number. Currently their D_2O has a tritium assay of 0.06 micro-curies/kg. D_2O (150 dpm/ml.).

I inquired about their past practises, e.g. 15 years ago. At that time they neither filtered nor redistilled the D_2O, but followed the same practises. All samples of D_2O from a given drum, presumably Savannah River D_2O, were given the same lot number.

The statements by Merck, Sharpe & Dohme describing their procedures should not lead to the variability of tritium in a given lot number reported by Wurden et. al.

1. H.-S. Bosch, G.A. Wurden, J. Gernhardt, F. Karger and J. Perchermeier, IPP III/149, July 1989. Max Planck Institut für Plasmaphysik, Garching bei München. "Electrochemical "Cold Fusion" Trials at IPP Garching. Manuscript sent to ERAB Cold Fusion Panel, 26 July 1989 See also G. Kreysa, G. Marx and W. PLieth "A Critical Analysis of Electrochemical Nuclear Fusion Experiments", submitted to J. Electroanal. Chem. 26 April 1989 and qouted by Bosch et. al.

xc: ERAB Panel

BRIGHAM YOUNG
UNIVERSITY

THE GLORY OF GOD
IS INTELLIGENCE

September 11, 1989

John R. Huizenga
Energy Research Advisory Board to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585

Dear Mr. Huizenga:

Re: Information for ERAB Panel on Cold Fusion

Your letter dated 9 August 1989 seeks information regarding electrolytic cells using solutions of LiOD in D_2O. We have done such experiments at BYU this summer in cells built and operated by Profs. W. Pitt and J. Harb. The mass inside each cell is isolated from the outside environment. Information is attached regarding these experiments and others performed by BYU researchers. None show evidence for tritium or "excess heat" production.

The cells described in our Nature paper (Nature **338**: 737, April 27, 1989) did not involve LiOD but rather an acidic solution including various metal salts. A recent paper submitted to the Journal of Fusion Energy for the Sante Fe Workshop on Cold Fusion Phenomena (attached) provides additional information regarding these electrolytic cells. Figure 1 displays an SEM photomicrograph taken by John Hack of Yale University of the fused titanium material used in the BYU cells. We have not looked for tritium production in these cells. We have, however, seen evidence for 2.5 MeV neutron production as described in the attached paper. Similar results were obtained in an experiment conducted in cooperation with Italian colleagues in the Gran Sasso Laboratory in Italy (to be published in Il Nuovo Cimento).

Evidence for neutron production in both electrolytic cells and deuterium-gas charging experiments has also been found in experiments conducted jointly with Howard Menlove and associates at the Los Alamos National Laboratory (see attached paper, submitted to Nature). The LANL/BYU collaboration was established during a visit by Steven Jones to LANL in April 1989. During the visit, Jones encouraged the use of deuterium-charging techniques which had been used at BYU since 1986, but without cooling the samples. The cooling approach was started by Scaramuzzi in Italy.

The LANL/BYU experiments have shown that neutron bursts occur during warm-up from liquid nitrogen temperatures, with the highest frequency of bursting at approximately

DEPARTMENT OF PHYSICS AND ASTRONOMY
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-30 C (see paper by Menlove et al.) We call attention to the fact that titanium alloys, as used in these experiments, have been found to undergo rapid hydriding from hydrogen in solution in the lattice with accompanying crack growth, with a maximum in the hydriding/crack growth rate observed at approximately -50 to -30 C as shown in Figure 2 (from W. J. Pardee and N. E. Paton, "Model of Sustained Load Cracking by Hydride Growth in Ti Alloys," Metallurgical Transactions A, **11A**: 1391-1400, 1980. This paper was pointed out to Steven Jones by John Hack of Yale.) We are trying now to determine whether there is a correlation between hydriding or cracking in the Ti alloy and neutron production, as both seem to occur at around -30 C.

Another experiment to look for "cold fusion" neutron production involves Steve Jones and Moshe Gai of Yale, along with others including Kelvin Lynn of Brookhaven National Lab. We will provide the committee with results from the Yale/BYU/BNL experiment, which took data 22-29 August 1989, when the off-line analysis is completed.

We hope this information will serve the Panel on Cold Fusion and will be happy to provide more information as requested.

Sincerely,

Steven E. Jones by BKH

Steven E. Jones
Physics and Astronomy Department

D N Bennion

Douglas N. Bennion
Chemical Engineering Department

B. Kent Harrison

B. Kent Harrison
Director, Center for
Cold Nuclear Fusion Studies

SEJ/BKH/DNB:nlh
Enc.
✓ cc: John Woodard, DOE
cc: Jacob Bigeleisen

Items 1-8 in the following address the points listed in the 9 August 1989 letter from John R. Huizenga to Steven Jones.

1. Source and specific activity of D_2O used in the electrolytic cells.

2. Description of the electrolytic cell, including volume of D_2O.

3. Cell operating conditions.

4. Schedule of additions of D_2O during operation of the cell.

5. Tritium analyses:
   a. Method of purification of samples for analysis
   b. Analytical method
   c. Schedule of cells examined for possible tritium production
   d. Specific activity of D_2O, $D_2(g)$ and PhD_n as a function of time for each cell.

6. Neutron production rate in each cell or in comparable cells.

7. Heat balance measurements in each cell in which tritium is found or in comparable cells.

8. What tritium sources are located in the buildings where the electrolytic cells are operated and/or in the analysis stages.

BYU

The following is in response to questions posed by the Panel of Cold Fusion of ERAB in a letter to Dr. Steven Jones from John R. Huizenga dated August 9, 1989.

1. The D_2O used the electrolytic cells was purchased from Aldrich with a purity of 99.9%. There was, however, a small amount of H contamination caused by short exposure to the atmosphere.

2. A rough schematic diagram of the cell design is shown in Figure 1. The host metal electrode is a cylindrical rod surrounded by a concentric counter electrode. The electrodes are contained in a glass cell surrounded by an evacuated glass jacket. Although precise calorimetry measurements are not the main goal of this work, conductive and convective heat losses must be minimized in order to measure the temperature rises which may accompany the fusion. Any temperature rises will be correlated with other signs of fusion such as neutron production.

A key feature of this cell is that mass inside the cell is completely isolated from the outside environment. This isolation is made possible through the use of a recombination chamber which provides a platinum catalyst on which the oxygen and deuterium (or hydrogen, in reference "blank" experiments) may recombine; excessive pressure build-up owing to O_2 and D_2 is thus eliminated. A Nafion™ membrane is placed between the electrodes to prevent O_2 and D_2 bubbles from contacting respectively the host metal cathode or the anode. The evolved gases exit the cell separately, pass through separate flow meters, and then recombine in a chamber filled with a Pt catalyst. The volume of the recombined water is measured and the water cooled to the room temperature before it is returned to the glass cell. Recombination is performed externally to the glass cell so that the heat evolved in recombination will not build up in the electrolyte. Gases remaining in the cell and recombination chamber are collected and analyzed quantitatively in collaboration with Bruce Jackson of the Chemistry department at Brigham Young University for fusion products. This includes mass spectrometry on an instrument capable of distinguishing ^4He and $^2D_2$. Key cell features are summarized in the list that follows:

a. Separated Electrode Compartments (membrane);
No in-cell recombination or burning of Pd.

b. Closed System with External Recombination;
No escape of fusion products. We can analyze both the gas and the electrode.

c. Palladium anode; 3M LiOD

d. Temperature measurement-lower limit of detection is between 0.1 and 0.25 W.

e. Data acquisition system which displays and continuously records gas flow rates, temperature (3 locations), pressure, and cell voltage. Experiments are performed at constant current.

f. Concentric geometry with uniform current density at cathode.

3. The operating characteristics of the experiment were as follows: 500 mA, 600 A/cm^2, approximately 3.55 V.

4. In our experiment we externally recombined D_2O and returned it directly to the cell. Due to hold up in the recombination chamber we were forced to add small volumes of make-up D_2O (less than 0.5 ml) in 8 hour intervals.

5. The tritium analyses was as follows:

a. There was no purification of the samples. HCl was added to neutralize the sample to pH 6.

b. 2.5 ml of neutralized solution was added to 10 ml of aquasol scintillation cocktail. The solution was shook well to ensure proper mixing. Placed in beta-mate scintillation counter. 3 count-measurements were taken for 10 minutes each.

c. All cells post mortem.

d. All cells at background scintillation levels of approximately 30 d/min·ml.

6. The efficiency of the neutron detector was approximately 1%. The single neutron rate was 2.18×10^{-1}/sec ($\pm 5.86 \times 10^{-3}$) which was 5.9 standard deviations above background. The average single neutron rate appeared to increase slightly throughout the experiment with D_2O (LiOD); however, the light water run remained relatively flat. The coincident neutron rate (burst rate) was 6.61×10^{-3}/sec ($\pm 1.02 \times 10^{-3}$) which was 3.4 standard deviations above background.

7. We have not seen tritium levels above background.

8. The only other source of tritium was tritiated glycine, which we used as standards.

Experiment at BYU
by Bill Pitt + John Harb
Summer 1989
(with Jones group)

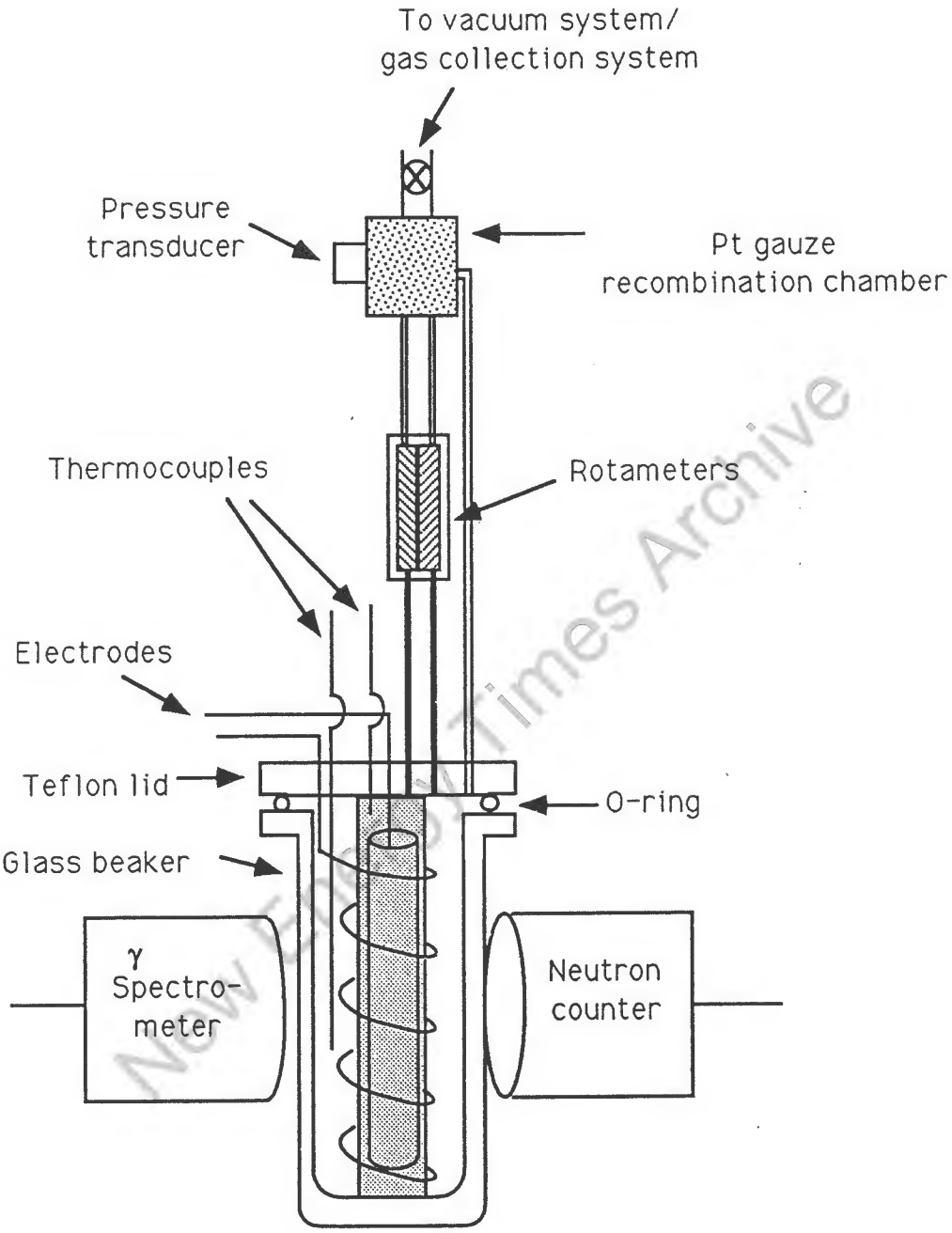

Figure 1. Schematic of Fusion Cell

Response to ERAB request for information in letter from John R. Huizenga dated August 9,1989, to Douglas N. Bennion. He requested that the response be sent to William Woodard and to Professor Jacob Bigeleisen regarding tritium and a more general report directly to Dr. Huizenga.

1. D2O source:
(a) 3000 ml from Isotec Inc. (A Matheson USA Company) 3858 Benner Road, Mianisburg, Ohio 45342 (513) 859-1808.
(b) 1000 ml from Aldrich Chemical Company, Milwaukee, Wisconsin 53233.
Our large cell filled from source a. Smaller cells filled from both a and/or b.

2. and 3. Description of cell and operating conditions.
(a) The large cell holds roughly 1200 ml of solution. It has a gear pump, heat exchangers, valves, gas-liquid separation vessel, the cell itself, and a circulating hydrogen-oxygen recombination system. The circulating system is made of 316 stainless steel. The cell itself is made of Tefzel, a Teflon like material produced by duPont. The cell contains a cathode and anode facing each other separated by 0.1 cm. The area of each electrode is 0.1 cm^2. Electrolyte, 4 M LiOD in D_2O, is pumped through the gap between the electrodes at over 2000 cm/s. At this pumping rate, gas bubbles are removed rapidly. Currents of 22 amperes are possible at about 70 V applied potential.
(b) The small cell holds about 80 ml of solution. The electrodes are arranged with the cathode on top of the anode. The cathode is 200 titanium sheets held together by a rod and nuts at the top. The sheets are each 0.001 inches thick and about 2 cm by 3 cm. The lower edges of the stack face the platinum gauze at the bottom of the glass vessel. The glass vessel is jacketed for cooling water flow. The hydrogen and oxygen exit into a recombination flow system. A cooling water coil is located in the recombination loop to allow heat removal there. Recombined water is returned to the electrolytic cell. It operates at about 12 amperes at 7 volts.

4. Addition of D2O. Unless a leak develops, both systems are sealed except when samples are withdrawn so no addition of electrolyte is needed when the systems operate properly.

5. Tritium analyses.
(a) Samples are removed from the system, 1 M HCl added to adjust pH to about 6, and then mixed with scintillation cocktail. At this

time no additional purification seems needed. Background counts are about 30 counts/minute/ml. We have tried lithium chloride solutions instead of lithium deuteroxide. The resulting chlorine leads to chemoluminescence. Purification techniques are now being worked out for the chloride case. The plan is to use copper/and or iron powder to react with residual chlorine and hypochlorite and then to distill all the D2O (and any DTO) into another flask before mixing with the cocktail.
(b) Method of analysis is to use a scintillation counter to detect beta induced scintillations.
(c) The large, high current density system was operated for about three days. Samples taken periodically have showed no tritium. The large, high surface area cathode cell is just now being started up. We should have results from this cell in the next few weeks.
(d) So far, we have seen no tritium.

6. Only the large, high current density cell has been tested for neutron activity using the Czirr-Jensen neutron detector (J. Bart Czirr and Gary L. Jensen, "A Neutron Coincidence Spectrometer," accepted for publication in *Nuclear Instruments and Methods*.). No neutron activity was detected during a single run of about 1.5 days.

7. To within our rather rough heat balance capability, no excess heat has yet been seen in either cell. Input to the large cell was roughly two kilowatts and our accuracy was roughly plus or minus 50 to 100 watts. The small cell has an input of about 70 watts with an accuracy of measurement of plus or minus 5 watts. Improved heat balance measurements will increase the accuracy on the small cell to plus or minus 0.5 watts.

8. To the best of my knowledge, there are no tritium sources located in the buildings where tests or analyses are being made. If we ever see any positive results, a thorough investigation will be made to determine the possibility of any such sources.

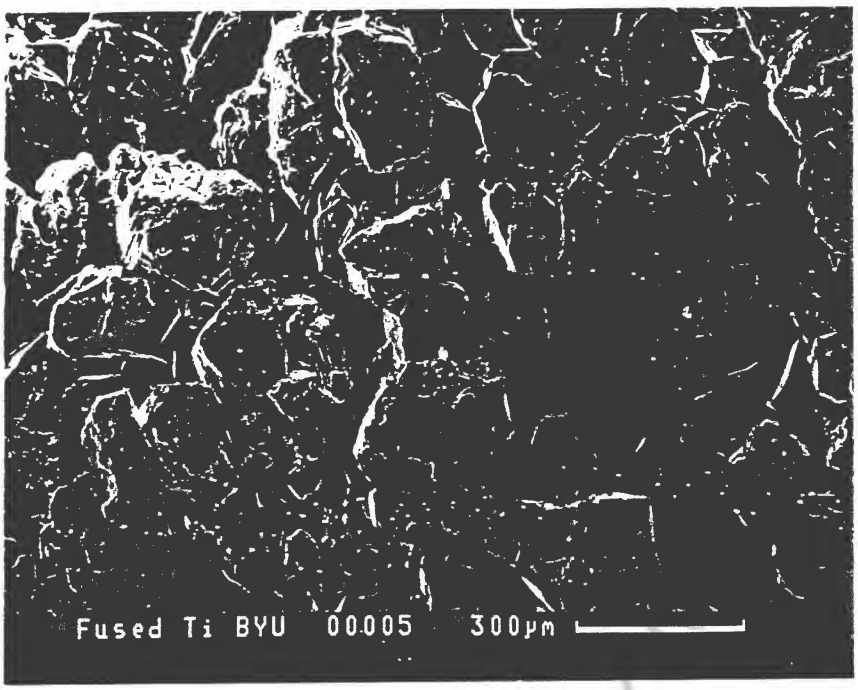

Fig. 1

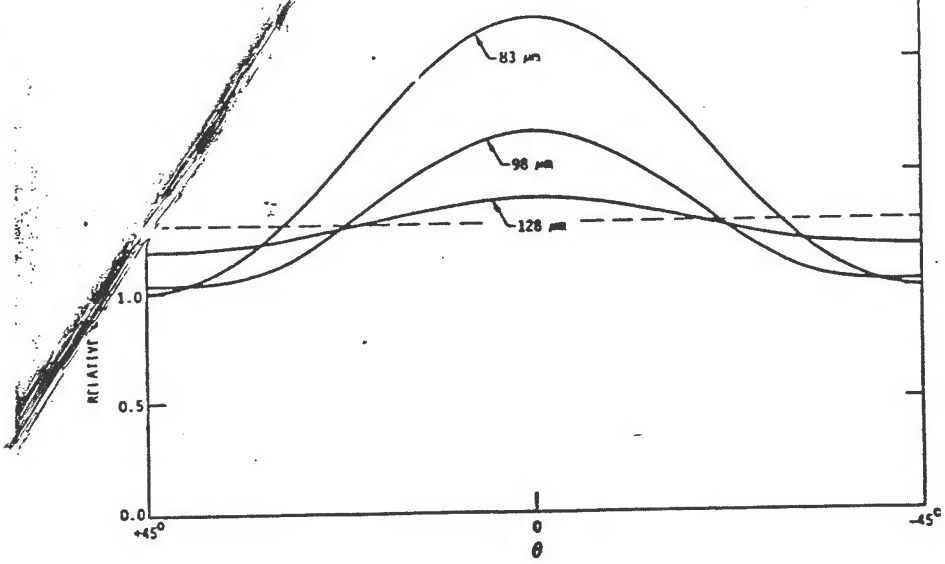

Fig. 3—Relative hydrogen concentration after 10 h at 20 °C as a function of angle at three different distances from the crack tip. Dashed line is bulk concentration before application of stress. All other parameters as from Fig. 2.

The angular dependence in a 90 deg cone in front of the crack tip is shown in Fig. 3 for three different radii. The dramatic peaking in front of the crack tip emphasizes the importance (except possibly for short growth periods at low temperature) of retaining the angular dependence in the diffusion equation. The hydrogen distribution decreases moderately rapidly to the bulk value with increasing distance from the plastic zone boundary.

Figure 4 displays the crack growth rate at 10 h as a function of temperature. The physical explanation of the peak is clear. Crack growth rate is diffusion limited at low temperatures and thermodynamics (solubility) limited (the hydride is no longer stable) at high temperatures. The phenomenon is analogous to temper embrittlement of steel, with the crack growth rate shown here analogous to a vertical slice through a temperature-time family of ductile-brittle transition temperature curves. The crack growth rate here (hydrogen assisted sustained load crack growth) corresponds to the time derivative of DBTT (or the adsorption on the grain boundary). Preliminary experimental results[19] indicate that this (theoretical) transition temperature is 20 to 40 deg too high, as shown in Fig. 5. That is, experiment seems to indicate a barrier to

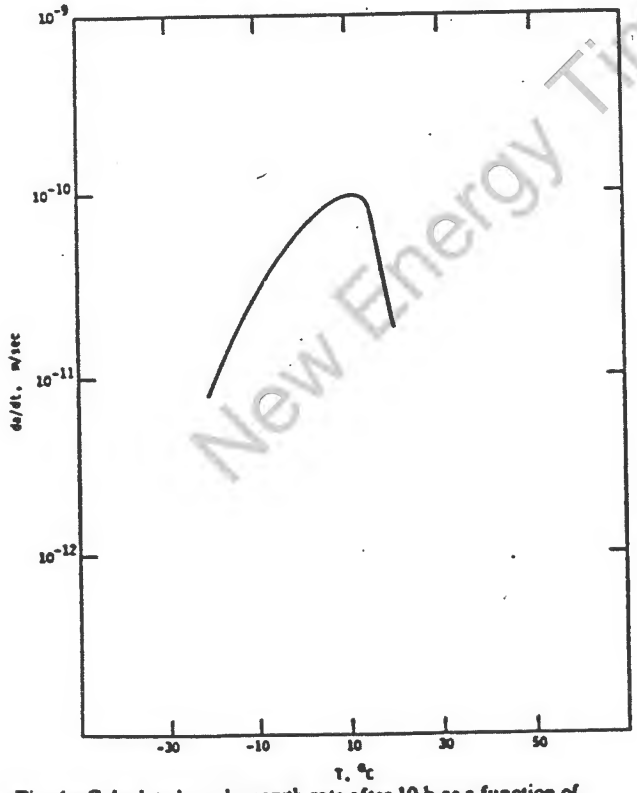

Fig. 4—Calculated crack growth rate after 10 h as a function of temperature. Parameters as for Fig. 2.

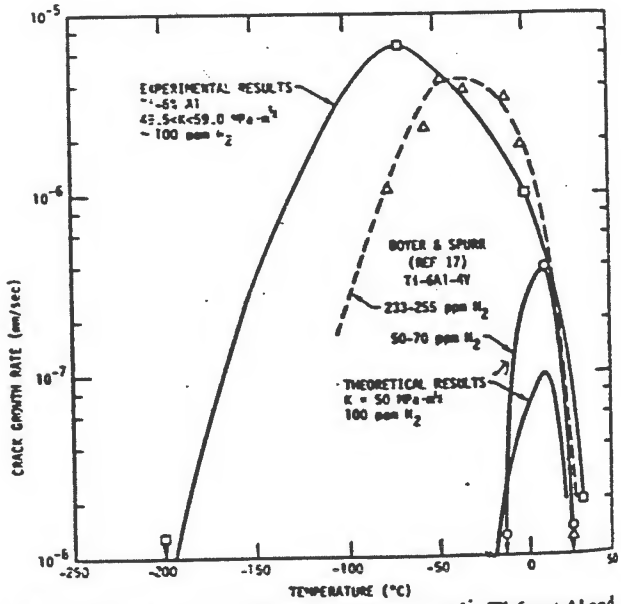

Fig. 5—Crack growth rate plotted *vs* temperature for Ti-6 pct Al and Ti-6Al-4V (Ref. 17). Experimental and theoretical results have similar functional behavior, but the theoretical peak is smaller and occurs at higher temperature.

Fig. 2

MATERIALS USED IN COLD FUSION EXPERIMENTS

PRINCIPAL INVESTIGATOR: Howard Menlove, gas cylinders prepared by Michael Paciotti, Steve Jones and Ed Garcia

ORGANIZATION: LANL groups N1 and MPDO with BYU (Jones and students)

SHORT DESCRIPTION OF TYPE OF EXPERIMENT:

Degreased Ti alloy turnings were exposed to clean D_2O gas at up to 800 psi. Pressure cylinders were placed within high efficiency neutron detectors. Electronics records time correlated neutrons.

RESULTS AND COMMENTS:

Neutron bursts seen within a time window of 50 microseconds from Ti alloy turnings exposed to clean D_2 gas. Bursts of $\sim 10^2$ emitted neutrons observed.

Random neutron singles have also been seen at an average rate of approximately 0.1 neutrons/second.

Clean surfaces and D_2 gas felt important, but if overdone, exothermic deuteriding of metal is seen at temperatures $< 200^0C$.

Moderate deuteriding of metal ($D/Ti \leq 0.4$) appears to enhance the effectiveness of thermal cycling in producing neutrons.

BYU (D_2 + H_2) gas charging experiment, for ^3He.
Sample prepared 6 July 1986
Sent to H. Craig for ^3He and ^3H analysis 25 March 1988,
Sent on to A. O. Nier for final analysis in 1989.

MATERIALS USED FOR GASEOUS CHARGING EXPERIMENTS (please complete one sheet for each experiment)

MATERIAL	Nickel, strips, surfaces buffed
PURITY	?
ALLOYING ELEMENTS	?
SOURCE OF MATERIAL	B.Y.U. Stockroom

PREPARATION
CAST OR WROUGHT
ANNEALED
ATMOSPHERE
VACUUM
SPECIAL TREATMENT
Vacuum baked at up to 800°C for ~1 hour then 825°C for ~1 hour. (60% D_2 +40% H_2) at 70 p.s.i. applied to sample at 825°C for 45 min., then gas pressure maintained while sample cooled.

CHARACTERIZATION
STRUCTURAL
CHEMICAL
BEFORE OR AFTER USE
METHODS Gas-charging; mass spec. analysis for ^3He.
RESULTS
Sample tested in sensitive mass spectrometer operated by Prof. Alfred O.C. Nier (Univ. of Minnesota). ^3He content <30,000 atoms, which would also imply a low limit on ^3H since ~2.5 years had elapsed between deuterium-charging and ^3He analysis.

NOTABLE
OBSERVATIONS

D/METAL RATIO ATTAINED

EXPERIMENT YIELDED HEAT	_____YES	_____NO
NEUTRONS	_____YES	_____NO
TRITIUM	_____YES	___X___NO
HELIUM	_____YES	___X___NO

LANL/BYU experiments
NOTE: Same material (Ti-6Al-6V-2Sn) is used in targets prepared by Jones, Hack and Zilm for Yale/BYU/BNL experiments performed 22-28 August 1989, and in experiments performed at BYU during Summer 1989.

MATERIALS USED FOR GASEOUS CHARGING EXPERIMENTS (please complete one sheet for each experiment)

MATERIAL Titanium alloy: Ti 6Al-6V-2Sn also Ti-6Al-4V (no spec. sheet)
PURITY

ALLOYING ELEMENTS Al 5.6%, V 5.4%, Sm 1.9%, Fe 0.58%, O 0.18%, C 0.02%, N 0.01%, H 0.0007%, Cu 0.54%.

SOURCE OF MATERIAL Precision Rolled Products Inc.
(Used at BYU in hydraulic presses)

PREPARATION
CAST OR WROUGHT
ANNEALED Annealed AMS 4971C
ATMOSPHERE D_2 at 40 - 80 atm.
VACUUM
SPECIAL TREATMENT Degrease lathe turnings with methylene chloride, then methanol, then pure water. Sorbtion pumping and baking at 80-100^0 C.

CHARACTERIZATION
STRUCTURAL Heavy cold working due to turnings
CHEMICAL
BEFORE OR AFTER USE Loss of strength for D/Ti > 0.4
METHODS
RESULTS

NOTABLE OBSERVATIONS Bursts of neutrons seen at room temperature and during thermal cycles with highest frequency of bursts at (-30 $\pm$ 30)^{0}C. No correlations yet between neutron bursts and acoustic emission is very active. (LANL/BYU results - see Menlove et al. paper.)

D/METAL RATIO ATTAINED .05 to 1.4 (determined from change in D_2 pressure)

EXPERIMENT YIELDED HEAT	_____ YES	_____ NO
NEUTRONS	X YES	_____ NO
TRITIUM	_____ YES	_____ NO
HELIUM	_____ YES	_____ NO

LA-UR 89-1974 (Rev.

MEASUREMENT OF NEUTRON EMISSION FROM Ti AND Pd IN PRESSURIZED D_2 GAS AND D_2O ELECTROLYSIS CELLS*

H. O. Menlove, M. M. Fowler, E. Garcia,
A. Mayer, M. C. Miller, and R. R. Ryan
Los Alamos National Laboratory
Los Alamos, NM 87545

S. E. Jones
Brigham Young University
Provo, Utah 84602

ABSTRACT

We have measured neutron emission from cylinders of pressurized D_2 gas mixed with various forms of Pd and Ti metal. For some of the cases, the Ti was coated with a surface layer of Pd. The gas pressure ranged from 7 atm to 80 atm, and the Ti loadings ranged from 20 g to 200 g. Experiments also have been performed for D_2O electrolysis samples. The neutrons were measured using high-efficiency cavity-type detectors containing ^3He tubes. Random neutron emissions were observed as well as time-correlated neutron bursts. The time spread in an individual burst was less than 100 μs. For most of the samples, the neutron emissions were observed after the cylinders had cooled to liquid nitrogen temperature and were warming to room temperature. The bursts occurred about 40 m into the warm-up phase, and the random emission occurred for at least 17 h after the sample reached room temperature. The burst cycle could only be repeated a few times before neutron emission ceased. The neutron emission rates were very low and the 17-h random emission rate was 0.05-0.2 n/s; however, this yield was still highly significant. The instantaneous neutron bursts were more dramatic with yields several orders of magnitude above the coincidence background rates.

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*Work supported by the US Department of Energy, Office of Safeguards and Security.

I. INTRODUCTION

The recent announcement by Fleischmann et al.,[1] that excess heat and substantial neutron flux had been observed in Pd cathodes in electrochemical cells, stimulated numerous experiments, although several claims (for example, gamma rays from neutron capture) have been retracted. Independently, Jones et al.,[2] observed 2.5-MeV neutron production at low levels during electrolytic infusion of D_2 into Ti and Pd electrodes, and discussed other means of creating nonequilibrium conditions, which might lead to "cold fusion." A replication of the latter experiment in the Gran Sasso Laboratory in Italy[3] provided confirmatory evidence for low-level 2.5 MeV neutron production. In addition, cold fusion has been reported in Ti subjected to pressurized D_2 gas and temperature changes.[4] We now report neutron emissions at low levels in both electrolytic cells and in metals subjected to pressurized gases. In particular, we have observed the production of 10-100 n in bursts of ≤100–μs duration, as well as random neutron emissions.

As part of our investigations of the cold fusion phenomena and possible radiations from the samples, we have measured both burst neutrons and random neutron emissions from a variety of sample types. The samples included cylinders of pressurized D_2 gas mixed with various forms of Pd and Ti metal chips, sponge, crystals, and powder. In addition, we have performed neutron measurements for electrolysis cells containing D_2O and cathodes of Ti, Pd, and V.

We are using four separate neutron detector systems operated in parallel experiments. The detectors all utilize ^3He gas proportional counters embedded in a polyethylene (CH_2) moderator. Three of the detectors are of the cavity- (well-) type, and one has an open channel for larger samples. The electronics are based on shift-register circuits[5] that give both the random and time-correlated neutron counting rate.

II. DETECTORS

Four similar detector systems were used in the present experiments to increase sample throughput and to act as control experiments. All of the detectors were located in the same experimental laboratory with separation distances of 1 to 2 m. There was negligible crosstalk between the detectors because of the small yield of the neutrons and the large solid-angle coupling of each sample to its primary detector. The characteristics of the four neutron detectors are listed in Table I. The efficiencies were measured with a calibrated ^{252}Cf source (Av energy = 2.3 MeV). These compact high-efficiency detectors were developed at Los Alamos National Laboratory as part of the nuclear safeguards program.[6] Detailed descriptions of the detectors can be found in Refs. 7-9. These detectors are well suited for detecting the neutron burst emissions for the following reasons:

- The neutron slowing time jitter in the CH_2 gives an instantaneous burst of neutrons a time spread of ~40 μs (neutron die-away time in the detector related to neutron moderation, leakage, and capture).
- Four amplifier channels are used in each detector with electronic clipping time constants of ~180 ns.
- There is a derandomizing buffer storage[5] at the input to the shift-register electronics to reduce deadtime for burst events.
- All neutron counts trigger the time-correlation circuit without waiting through the gate time, thus reducing deadtime.
- High efficiency is required to detect a significant fraction of the small bursts.

Figure 1 is a photograph of the System 3 neutron detector with a pressurized D_2 gas cylinder being placed in the cavity. The five CH_2 (^3He tubes) pods on the circumference of System 3 can be used to determine the average neutron energy when sufficiently high neutron yields are

obtained. The energy measurement is derived from the ratio of the counting rate in the outside tubes (^{3}He) to the inside tubes. The ratio is a function of the average neutron energy because of the differential energy transmission through the CH_2.

The detector tubes for Systems 1, 3, and 4 are screwed directly into an aluminum metal cavity (see Fig. 1) that is sealed to prevent RF noise penetration, and desiccant is added to keep moisture out of the detector high voltage area. Four AMPTEK A-III hybrid charge-sensitive preamplifier/discriminators[7] are located inside this sealed cavity and they are placed at the base of the ^{3}He counters to eliminate analog signal transmission lines that are prone to pick up noise.

The time-correlated neutron counting[5] is essential for the neutron burst results reported in this paper. Every neutron count that enters the circuitry triggers the time-correlation counters that check if there are any other neutron counts within the selected time gate. We are using a coincidence time gate of 128 μs and this corresponds to about 3 times the neutron die-away time of the detector.

The time-correlated (coincidence) rate R is related to the number of neutrons (N) that are counted in the gate by the relationship

$$R = \frac{N(N - 1)}{2} .$$

Thus if 100 n are emitted from the sample in a burst of <100 μs, our System 3 would detect ~34 n and R would equal 561. For our larger bursts, we have observed N from the increment in our totals (singles) scaler, and the calculated R using the above equation has agreed with the observed R in the coincidence scaler. If the burst event lasts more than 128 μs the above equation will not be valid. The accidental coincidence counts are measured by sampling the shift registers after a delay of 1 ms following each neutron pulse.

III. DATA SURETY

For low-level neutron counting and especially neutron burst counting, it is difficult to distinguish true neutron counts from spurious background noise. We have taken the following measures to assure that our burst events originate form neutrons:

- We use four separate detectors operating in parallel to pick up common sources of noise such as line voltage spikes, RF interference, cosmic-ray showers, and external room neutrons.
- We alternate dummy runs with the active sample runs.
- For System 3, we split the signal output to long (128-μs) and short (16-μs) gates and require that the gate count ratio be consistent with the detector die-away time (~40 μs).
- We collect singles counts, coincidence counts, and accidental counts and require that the three rates be consistent with neutron bursts.
- We detected the neutron bursts at a predictable time (2000-4000 s into LN warm-up) and temperature (~30°C) for our first 12 bursts (samples Ti - 1 and Ti - 6).

We calibrated the gate fraction for instantaneous neutron bursts (spontaneous fission) using a ^{252}Cf source for which the ratio was 128-μs gate/16-μs gate = 3.18 ± 0.01. For the sum of several weeks of data collection in System 3, the long to short gate ratio was 3.2 ± 0.3 for the cosmic-ray background and 2.5 ± 0.4 for the sample neutron bursts. These ratios both agree statistically with the ^{252}Cf ratio and give additional evidence that the observed bursts are from neutrons. Cosmic-ray spallations give a source of instantaneous coincidence background neutrons, so the gate ratio should be the same as a ^{252}Cf spontaneous-fission source.

Prior to the work reported in this paper, we operated detector System 1 and System 3 for four weeks measuring Pons-type[1] electrolysis cells. During these experiments we observed no

neutron bursts or excess random neutron emissions. In retrospect, these samples could be considered dummy samples.

Additional measurements were performed to ensure that environmental noise was not getting into the detector systems. The results of these tests were as follows: (1) no gamma-ray sensitivity up to 1 R/h, (2) the detector efficiency was stable for temperatures as low as -40°C, (3) no electrical noise pickup for noise generators (Tesla coils) placed directly into the sample cavity, (4) a long-term efficiency stability (precision) of 0.01%, and (5) no microphonic noise pickup for mechanical fracturing experiments in the sample cavity.

IV. MEASUREMENT PROCEDURES

A. Gas-Type Experiments

The Ti and Pd samples used in the D_2-gas-type experiments were contained in a pressurized stainless-steel gas cylinder with a volume of 250 cm^3 and an empty weight of 1 kg. After evacuating the sample for about 1 h at 150-200°C, the cylinder was backfilled with D_2 gas at room temperature and sealed.

The sealed sample was then put through a liquid nitrogen (LN) temperature cycle. Typically, the LN cooling would last for 20-60 min. The sample was then removed from the LN and placed in the neutron detector cavity and allowed to warm to room temperature.

The measurement time bins were typically 1000 s or 2000 s; however, longer time intervals were used for some of the overnight runs. A complete temperature cycle would take about one day for most cases. Some cycles were shorter, lasting ~5 h. A given sample cylinder would be put through 7 to 14 of these cycles. The neutron emissions always ceased after a few cycles. Several of the samples were counted during the cooling down phase of the LN cycle with negative results.

One of the gas cylinders (DH-1) contained both D_2 gas (40 atm) and H_2 gas (40 atm) for a total of 80 atm.

B. Sample Material

At this early phase of the investigations, we have focused on experiments that contained a wide variety of material forms to maximize the chance of getting a neutron yield. The samples that gave neutron emissions contained mixtures of Ti and Pd turnings, sponge, foils, crystals, and powders. At least some of the material had been used in electrolysis experiments of the Jones type.[2] Twelve gas cylinder samples have been used in the experiments and five have yielded neutron emissions. Our attempts to run experiments using a single material component to isolate the neutron source have been unsuccessful except for one case. This isolation is very difficult because of the nonreproducibility of the effect. That is, the "right material" category might still give a negative result because it lacked the special feature required for the emission of neutrons. An exception to this isolation problem was sample DH-1 that contained only Ti alloy (6% Al, 6% V, and 2% Sn) turnings.

Table II gives a listing of the contents of the gas samples that we have measured this far.

The dummy sample for the D_2 gas experiment was a gas cylinder with identical size and mass to the normal samples. The dummy was air filled and it contained about 50 g of metal chips. For the electrolysis experiments, the dummy was six D_2O cells of the same size and mass as the samples runs but the dummies contained no electrodes. The detectors give the same background rate for the dummy sample and an empty sample cavity.

C. D_2O Electrolysis

In addition to the gas phase experiments, we have run four experiments using Jones-type[2] cells and electrolytes. Each of the experiments had six D_2O cells located in System 1. The anodes were gold foils and the cathodes varied with Ti, Pd, V, and Zr (foils, crystals, sponge, and sintered powder).

For one experiment, the electrolyte was D_2O mixed with the multiple ingredients described in Ref. 2. For the other three experiments, the electrolyte was an acidified (pH = 4) 10-g/L Li_2SO_4/D_2O solution. The currents and voltages were varied over the range from 0-4 A and 0-16

V, respectively. The data were collected in 1000-s or 2000-s time bins, and the experiments lasted for several days.

During the electrolysis experiments, simultaneous D_2 gas-type experiments were in progress in Systems 3 and 4.

V. RESULTS

A. Gas Phase Experiments

1. Burst Results. The first burst-type neutron emissions were observed from the gas-filled cylinder Ti-1. Most of the burst results give off too few neutrons to be measured in the uncorrelated (random) counting mode.

Cylinder Ti-1 underwent two LN temperature cycles without measurable neutron burst emissions. However, on cycle 3, two bursts were counted as shown in Fig. 2. The correlated background rate is about one count every 2000 s. Two more bursts were measured about 5 h later on temperature cycle 4. The dummy cylinder was run through the same temperature cycle alternating in the detector (System 3) with Ti-1. The control counter (System 4) provided null results during the entire experiment. Six subsequent LN cycles on sample Ti-1 gave null results.

Figure 2 gives the coincidence results for Ti-1, including the two active cycles as well as the results for the dummy cylinder that was cycled alternating with Ti-1 in System 3.

The next several cylinders were loaded with single Ti and Pd components in unsuccessful attempts to isolate the material that gave the neutron emissions. After four samples with null results, another integral (multicomponent) cylinder was measured with burst yields during six LN cycles (4, 5, 6, 7, 8, and 9). The largest burst results gave 85 neutron counts representing a source term of 253 n.

A collection of the six active cycles with Ti-6 is shown in Fig. 3. Both Ti-1 and Ti-6 demonstrated a pattern of bursting during the third or fourth 1000-s time interval during the

warm-up. The frequency distribution for the bursts from samples Ti-1 and Ti-6 peaked at 2800 s into the warm-up cycle.

To help establish the cylinder temperature vs the warm-up time, the dummy cylinder was run through the LN cycle with a thermocouple temperature probe inside the cylinder at the general location of the Ti material. The most probable temperature for the neutron burst observations was approximately -30°C. This burst time occurs about 15 min before the frost coating on the cylinders starts to melt.

The significance of the relationship between the temperature and the neutron bursts is yet to be established. It might be related to phase changes in the metal or to other stress conditions. If Ti metal is deuterided to a sufficiently high level, it has the possibility of going through a phase transition between 77 and 300°K. However, none of the samples that had been predeuterided at elevated temperatures gave neutron yields. The possibility of electrical discharge from cracking mechanisms will be discussed in the summary.

2. Burst Results from D_2 and H_2 Gas Experiments. In addition to the burst results described in the previous section, we have observed smaller bursts from a different type of experiment. In this experiment, the cylinder (DH-1) was loaded with 40 atm of D_2 plus 40 atm of H_2 gas. The addition of the H_2 gas was motivated by the possibility of obtaining p + d fusion in future experiments and measuring the high-energy (5.4-MeV) gamma rays.

This sample gave no bursts during the warm-up from LN temperature; however, we have observed at least four neutron bursts from the cylinder at room temperature. The first burst occured about 18 h after the first LN cycle. These data are shown in Fig. 4, where the top curve corresponds to the sample data and the bottom curve corresponds to the dummy cylinder run in the same detector (System 3). The control detector (System 4) was in operation during both the sample and dummy runs, and no bursts were observed in the control experiments.

3. **Random Neutron Emissions.** In addition to the burst-type results, we have measured random neutron emissions from two of the gas cylinders. The electronics that we are using were designed to separate purely random neutron emissions from time-correlated bursts in which two or more neutrons emitted at the same time are considered a correlated event.

Sample Ti-1 emitted random neutrons for at least a 17-h period while at room temperature. During LN temperature cycles 1 and 2, no burst neutrons were observed; however, when the sample was counted overnight (17 h) after cycle 2, we measured a yield of random neutrons. No time-correlation neutrons above the small background levels were observed during the 17-h period. Figure 5 shows the data from Ti-1 together with the data from the dummy cylinder collected the following night in the same detector. The data taken in the control counter (System 3) during the same two nights gave a constant background rate during the two nights. The average of the 17 h sample data was 0.4298 ± 0.0027 counts/s, whereas, average of 22 h of the dummy runs was 0.3943 ± 0.0022 events/s. The difference in the two average values corresponds to a 10.3σ significance level.

Immediately following the Ti-1 overnight run, in System 4, the sample (Ti-1) was counted alternating between Systems 3 and 4 with the dummy in the opposite detector. These runs were for 4000 s each with two round trips in each detector for a total of 8000 s in System 3 plus 8000 s in System 4. Sample Ti-1 gave an excess of random neutron counts in both systems and the combined (both detectors) significance for Ti-1 was 4.3σ above the dummy background baseline. No temperature cycling was involved during this period.

Sample Ti-3 gave an excess random neutron emission during the 5-h period following the first LN temperature cycle. The average totals rate was 0.2594 ± 0.0034 counts/s for the 5-h period which was 4.3σ above the dummy background baseline rate of 0.2413 ± 0.0025 counts/s that was measured for a 11-h period before and after the sample run. The adjacent control detectors gave a constant background rate during the sample and background time periods. Subsequent temperature cycling of this sample gave no random or bursts emissions.

For the random neutron emission results, long counting intervals are required to statistically differentiate the low-level emission rates (0.05-0.2 n/s) from the cosmic-ray background rate. In our experiments, we have looked for random emissions from all of the samples.

B. D_2O Electrolysis Results.

We performed three experiments with Jones-type[2] cells where each experiment involved six D_2O cells containing different cathodes of Ti, Pd, Zr, and V metal. While two experiments showed ~3σ results above background levels, the limited sensitivity in the random-counting mode precludes any definitive statement concerning neutron emission at this time.

A fourth experiment gave burst yields after running the current for about 12 h of electrolysis, and the bursts continued for several days. The time-correlation (coincidence) data from the detector (System 1) is shown in Fig. 6 (top). The burst activity continued for several hours after cutting off the current at 71 h into the experiment. We terminated the sample run to start the dummy run after 92 h. The background dummy run (six D_2O cells without electrodes) was made immediately after completing the sample run. These data are shown on the bottom portion of Fig. 6, and there is no evidence of burst events. The control runs taken in Systems 3 and 4 showed no burst activity or change in background rates during both the sample run and the dummy runs. The burst activity is evident for the sample run, and the largest burst corresponds to a source term of approximately 130 n. We are now trying to isolate which of the sample materials gave the neutron burst emissions.

VI. SUMMARY

We have observed both burst and random neutron emissions from samples involved in seven different experiments. Table III gives a summary of the results. The significance level of the random neutron results is shown in the bottom of Table III. Two different detector systems have

been used to measure the random emissions and all four systems have detected the burst results. The individual burst results are as much as 2 orders of magnitude above the background levels.

The results reported in this work do not define the neutron production mechanism. Several models have been proposed for the production of neutrons in the two types of experiments for which they have been detected; that is, electrochemical experiments and those in which various forms of Ti metals or alloys have been subjected to thermal cycling under D_2 gas at pressure. The possibility of particle acceleration due to change separation during a fracturing process has been suggested.[10-15] In support of the latter suggestion, both electron and positive particle emission have been observed at energies of several keV on a time scale consistent with our observed neutron bursts during the mechanical fracture of $TiD_{0.8}$.[15] Fracturing and fatigue of the samples has been observed after the samples were removed from the cylinders in most cases where neutron emission has been detected in our laboratory.

The random emission results *cannot* be explained as a large number of small bursts because sample Ti-1 gave approximately 2000 random neutron counts during the 17-h period with no net correlations (including neutron pairs).

Detailed characteristics of the materials that give the neutron emissions have not been established. It is difficult to isolate the material characteristics that are responsible for the neutron emissions because of the unpredictability of the emissions. Our most reliable source material for neutron burst yield is the Ti alloy (6% Al, 6% V, and 2% Sn). We have measured multiple bursts from this type material at room temperature. It is likely that the burst effect is not a property of the bulk material but it might be related to isolated dislocations in the samples. Also, there are probably different mechanisms responsible for the burst results and the random neutron emissions. The common denominator for all of the neutron emissions is that the samples are in nonequilibrium conditions such as temperature cycling and deuterium diffusion into or out of the samples.

We have not yet measured the neutron energies because the yields still are too low for the neutron spectrometry measurements. The neutron yields that we have measured in the present experiments are very low. For the highest random emission yields from Ti-1, the levels (0.1 n/s)

were similar to the yields reported by Jones et al.[2] However, our neutron burst results are too small to be detectable in the uncorrelated (random) neutron counting mode. For example, a neutron burst of 40 n measured during a 5-h experiment represents an average source term of 0.002 n/s.

Our future work will focus on the characterization of the material to obtain higher yields and to understand the neutron source mechanism. We will measure the neutron energy and repeat the experiments with H_2 and DT gas to help establish if the neutrons are originating from cold fusion, hot fusion (cracking and fractures), or some other source.

ACKNOWLEDGMENTS

The authors wish to acknowledge the useful discussions with M. P. Baker, G. W. Eccleston, P. A. Russo, and W. L. Bongianni during the early phases of these experiments. We are indebted to M. A. Paciotti for the preparation of sample DH-1 and to S. Taylor, J. Maxwell, and D. Mince for their assistance in sample preparation. The use of the high-efficiency neutron detectors with time-correlation circuitry developed under the support of the Department of Energy, Office of Safeguards and Security, was essential to obtain the results reported in this paper.

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FIGURE CAPTIONS

Fig. 1. Cylindrical detector System 3, consisting of 16 ^{3}He tubes in the central annulus plus 5 exterior CH_2 plus ^{3}He pods for background control and energy determination. A typical D_2 gas cylinder is being placed into the sample cavity. The shift-register time-correlation electronics package is shown beside the detector head.

Fig. 2. The combined coincidence results for the Ti-1 sample (left side) and the dummy cylinder (right side) that were measured in System 3. The dummy and the T-1 sample were measured alternating in time in the detector. The ordinate corresponds to the number of neutrons detected in the 1000 s time bins. These neutron bursts were ovserved to arrive in a burst of less than 100 μs.

Fig. 3. The coincidence results for six active cycles for sample Ti-6 measured in System 3. The neutron burst results mostly occur 2000-4000 s into the warm-up period.

Fig. 4. The number of coincidence counts vs time for sample DH-1 (top curve) and the dummy cylinder (bottom curve). All of the bursts occurred at room temperature. The ordinate corresponds to the number of time correlated counts measured during the 2000-s time bins. The background coincidence counts result from cosmic-ray neutron bursts in the detector. The first burst occured about 18 h after the first LN cycle.

Fig. 5. The totals (random) neutron counting rate for sample Ti-1 measured in System 4 over a 17-h time period at room temperature. The right-hand section of Fig. 8 corresponds to data for the dummy sample in System 4 and the time line is reset to zero when the dummy count starts.

Fig. 6. The time-correlated (coincidence) neutrons counts vs time for six D_2O electrolysis cells measured in System 1. The bottom data correspond to six D_2O dummy cells measured in the same detector after the sample run was completed. The ordinate corresponds to the number of time-correlated counts measured during the 2000-s time bins.

TABLE I

NEUTRON DETECTOR CHARACTERISTICS

Identification	Shape	Size	Number ^{3}He Tubes	^{3}He Pressure (atm)	Total Efficiency[a] (%)	Bkg (s^{-1})	Sensitivity Limit[b] (n/s)
System 1	Rectangular channel	25 x 35 x 35 cm^3	18	4	21	0.26	0.043
System 2	Cylindrical cavity	23 cm ϕ x 37 cm	6	4	26	0.10	0.022
System 3	Cylindrical cavity	22 cm ϕ x 35 cm	16	6	34	0.25	0.026
System 4	Cylindrical cavity	22 cm ϕ x 35 cm	16	4	31	0.39	0.036

[a]The total efficiency was measured using a calibrated ^{252}Cf source located at the sample position.

[b]Sensitivity limit corresponds to the source random emission rate for an 8-h count (3σ above background). For the burst-type events, the sensitivity is about 2 orders of magnitude better.

TABLE II

SAMPLE MATERIALS

Sample Identification	Neutron Yield	Gas	Pressure (atm)	Materials
Ti-1	Yes	D_2	40	100 g Ti sponge (no electrolysis); 4.2 g Ti (6,6,2)[a] turnings, 2 g Ti crystals 11 g Ti sponge (D_2O electrolysis)
Ti-2	No	D_2	20	~30 g Ti turnings (prior high temperature cycles)
Ti-3	Yes	D_2	20	25 g Ti turnings (D_2 loaded at LN temp.)
Ti-4	No	D_2	30	100 g Ti crystals, 100 g Ti sponge
Ti-5	No	D_2	40	46.4 g Ti metal turnings
Ti-6	Yes	D_2	40	147 g Ti pieces, 17.4 g Ti powder, 4.6 g Pd powder, (D_2O electrolysis--22.8 g Ti sponge, 1.7 g Pd pieces)
Ti-7	No	D_2	40	33.3 g Ti sponge deuterided to ~Ti $D_{0.18}$.
Ti-8	No	D_2	40	50 g Ti turnings, 4 g Ti sponge (both from D_2O electrolysis)
Ti-9	No	D_2	33	30 g Ti turnings and sponge (D_2O electrolysis)
Ti-10	Yes	D_2	40	23 g Ti turnings, ~75 g Ti sponge ~4 g Ti crystals, 2 g Pd foils (all from D_2O electrolysis)
Ti-11	Yes	D_2	40	82 g Ti powder, 10 g (90% Ti + 10% Pd) sintered in air
DH-1	Yes	D2+H2	80	~80 g Ti alloy (6,6,2)[a] turnings
Electrolysis 14B		Electrolyte D_2O/Li_2SO_4	NA	$cell_1$-V foils, $cell_2$-PdTi sintered powder, $cell_3$-Zr foil, $cell_4$-Pd foil, $cell_5$-PdTi sintered powder, $cell_6$-V foil. (D_2O/Li_2SO_4 electrolyte).

[a]Ti (6,6,2) corresponds to the alloy of Ti (6% Al, 6% V, and 6% Sn) by weight.

TABLE III

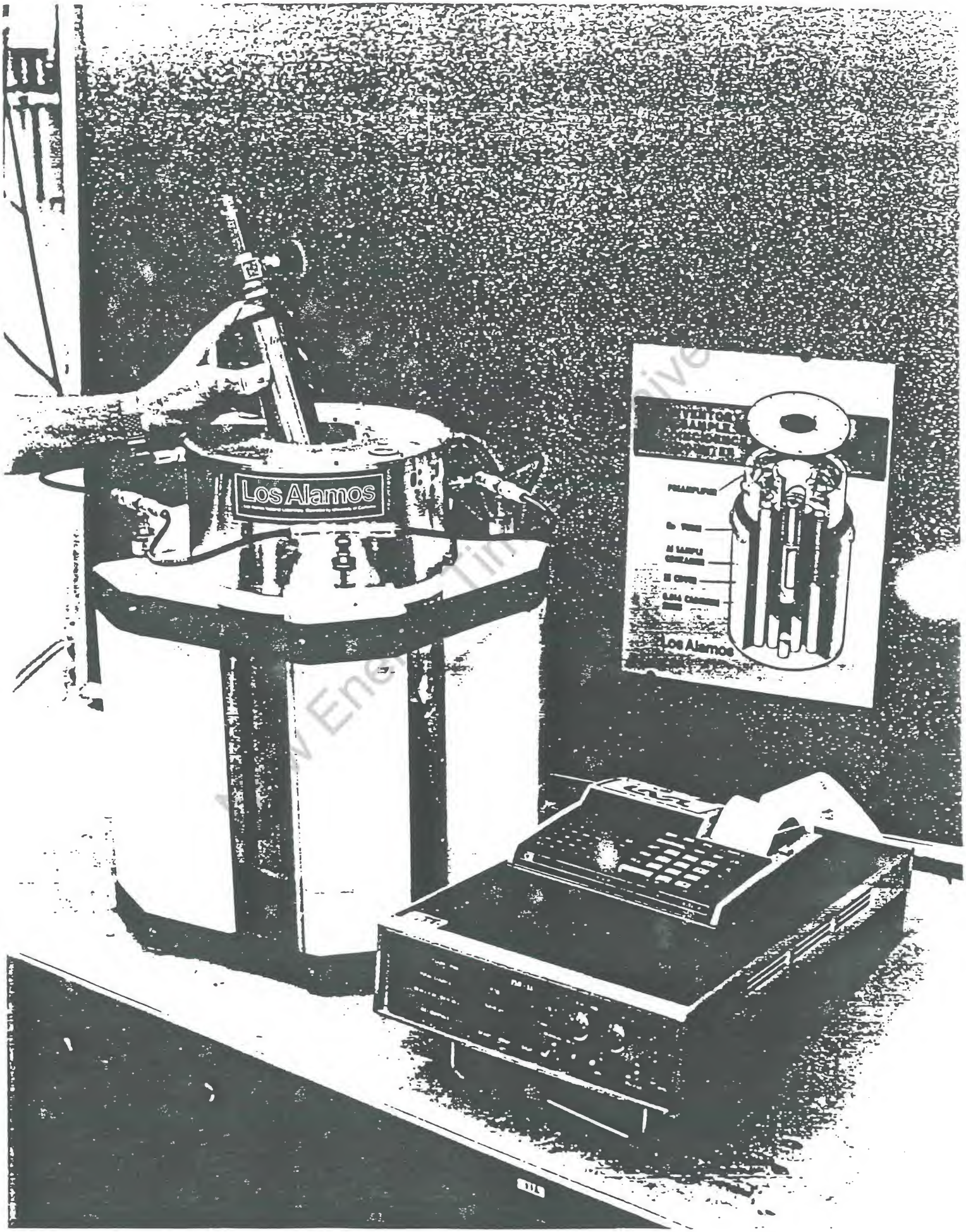
SUMMARY OF RESULTS

Neutron Burst Emissions

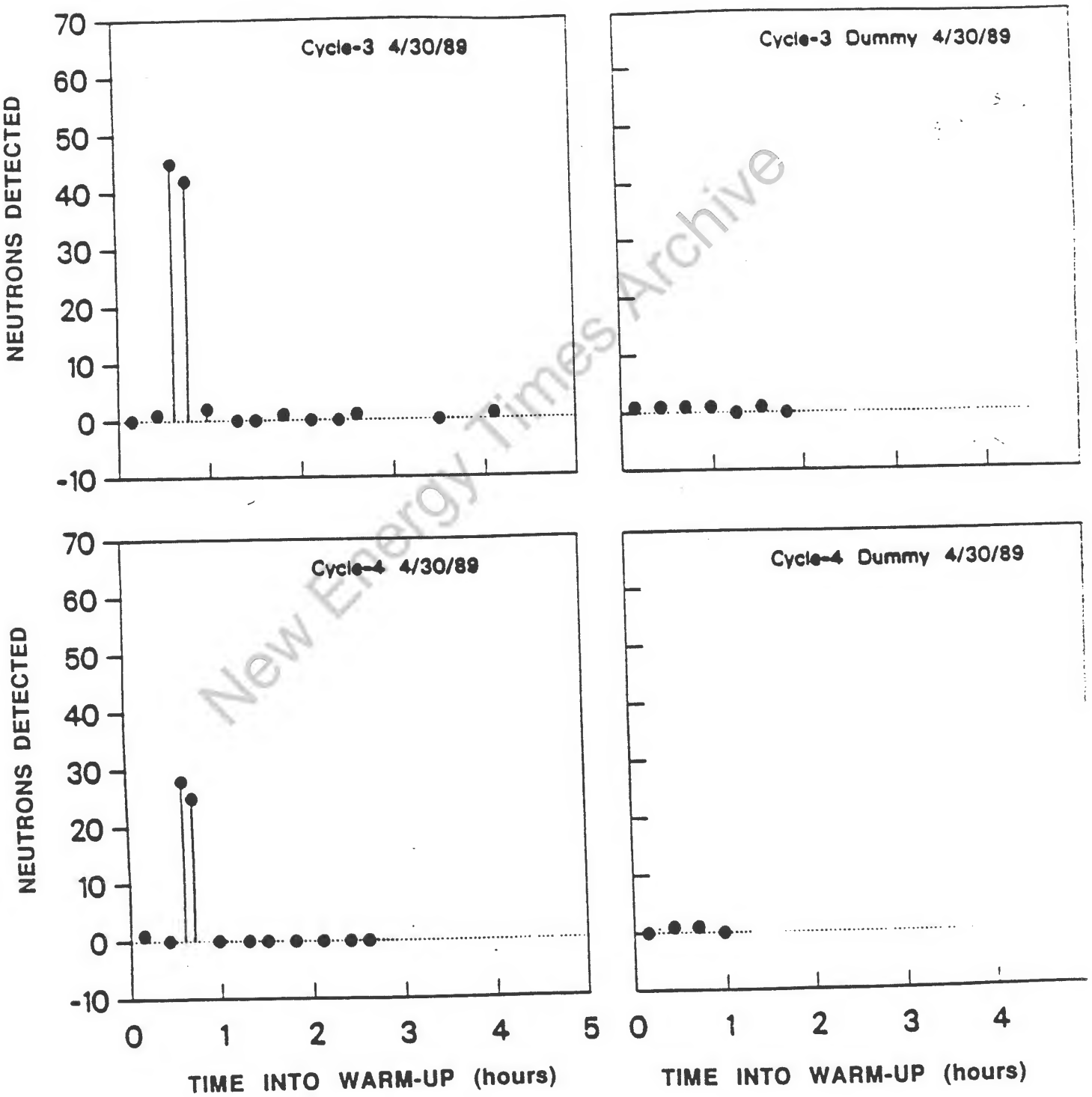
Sample No.	Number of Bursts	Burst Cycle
Ti-1	4	3,4
Ti-6	8	4,5,6,7,8,9
Ti-10	2	5,7
Ti-11	2	4
DH-1	5	1,2
D_2O/Li_2SO_4	6	NA

Random Neutron Emissions

Sample	Detector	Significance Level
Ti-1	System 4	10.3σ
Ti-1	Systems 3 and 4	3.9σ
Ti -3	System 3	4.3σ


Los Alamos
Los Alamos

TIME CORRELATED COUNTING DATA FOR SAMPLE Ti-1



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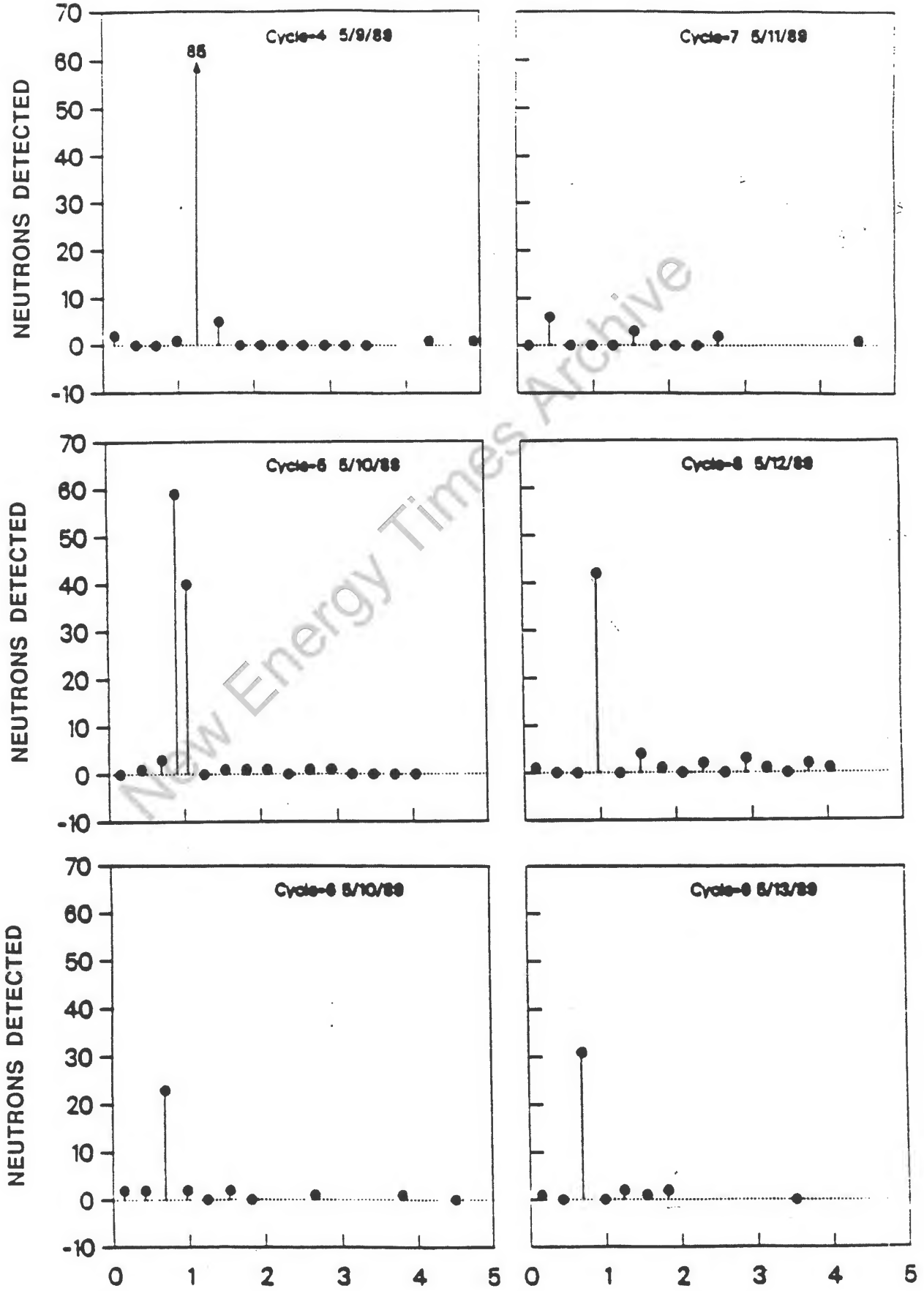

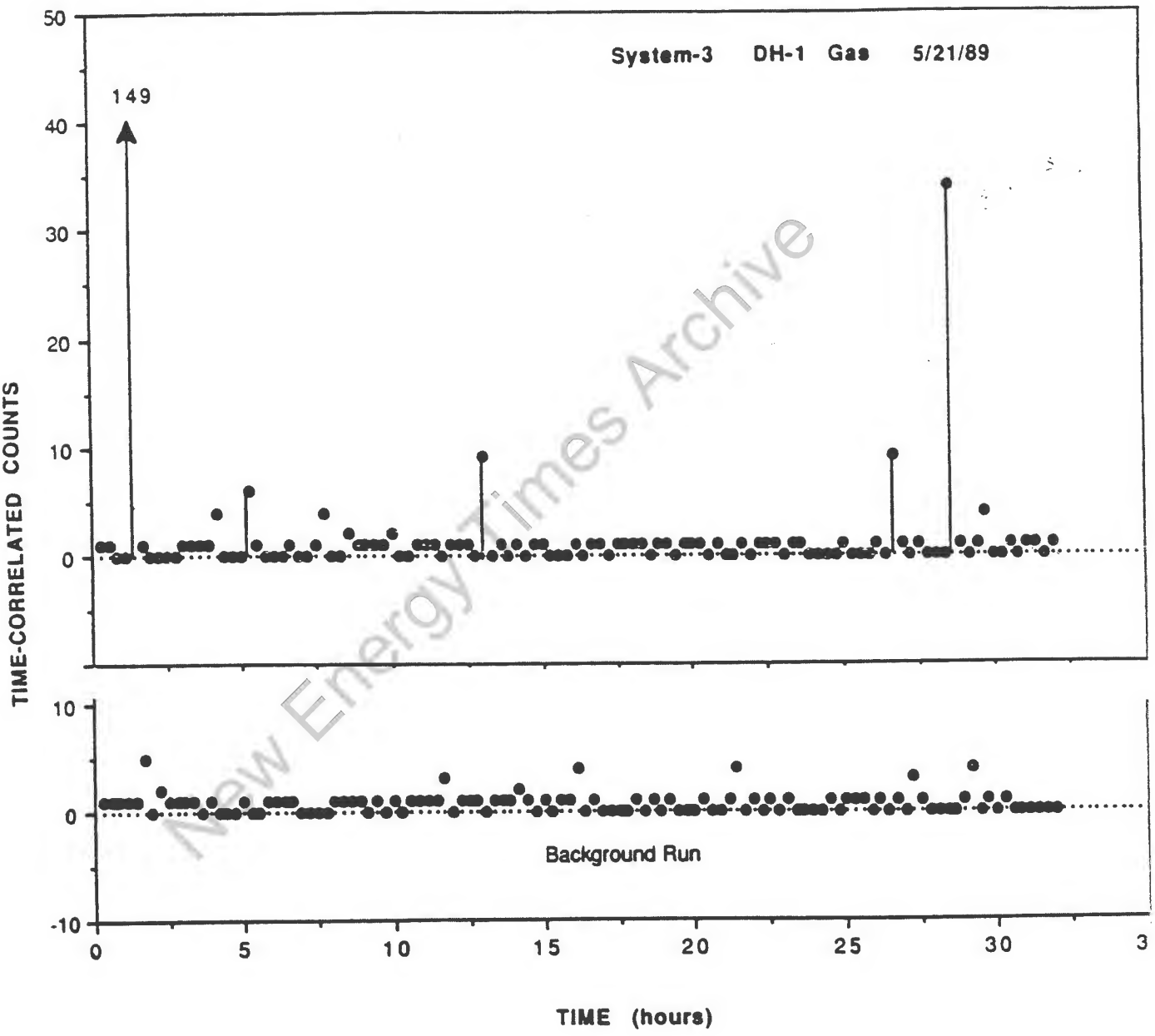

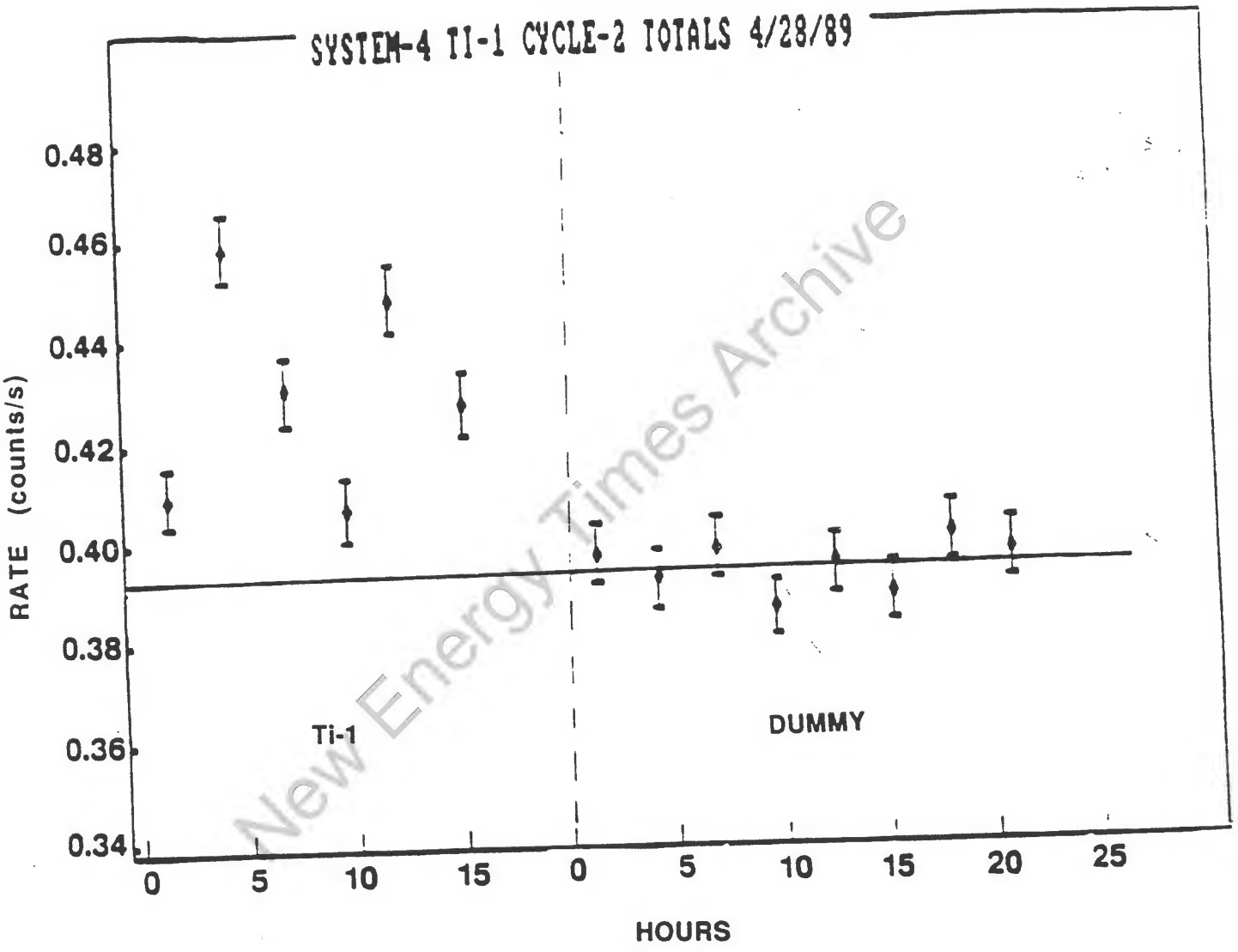

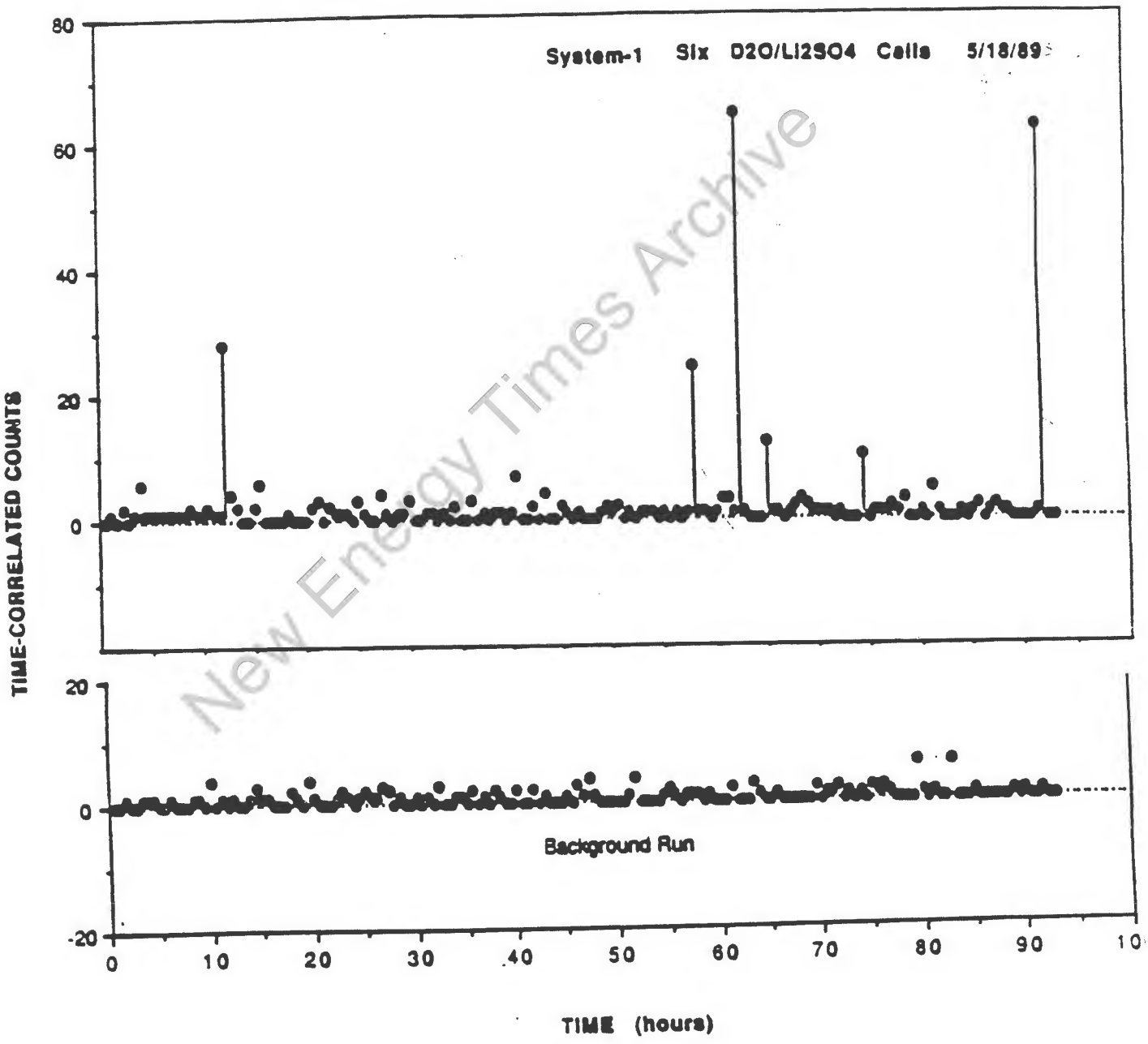

COLD NUCLEAR FUSION IN CONDENSED MATTER: RECENT RESULTS AND OPEN QUESTIONS

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Abstract

We have observed clear signatures for neutron emission during deuteron infusion into metals, implying the occurrence of nuclear fusion in condensed matter near room temperature. The low-level cold fusion phenomenon has been demonstrated in collaborative experiments at Brigham Young University, at the Gran Sasso laboratory in Italy, and at the Los Alamos National Laboratory. We have shown that cold fusion can be induced in metals using both electrochemical and variational temperature/pressure means to generate non-equilibrium conditions. Observed average neutron emission rates are approximately 0.04 - 0.4 n°/s.

Current efforts focus on trying to understand and control the cold fusion phenomenon. In particular, we wish to understand the correlation of fusion yields with parameters such as hydrogen/metal ion ratio, pressure (induced, for example, by electrical field or gas pressure or mechanical pressure), temperature variation, hydride phase changes, and surface conditions (e.g., a palladium coating on titanium). We want to know if the fusion arises due to the close proximity of the deuterons in the lattice (piezonuclear fusion), or rather from "microscopic hot fusion" accompanying strong electric fields at propagating cracks in the hydride. The latter interpretation would imply neutron emission in bursts. Our experiments show clear evidence for emission of $\sim 10^2$ neutrons in bursts lasting $< 50\mu$s, although random neutron-singles emissions were also observed. Experiments now underway to compare the d-d, p-d, and d-t fusion rates will be important to a consistent description of the new phenomenon. Careful scrutiny of this effect could increase our understanding of heat, helium-3, and tritium production in the earth and other planets.

For Proceedings of Santa Fe Workshop on Cold Fusion Phenomena,
(Submitted to Journal of Fusion Energy for Proceedings)

Introduction

Our research on cold fusion in condensed matter grew out of our work on muon-catalyzed fusion, and it is instructive to briefly revisit this other form of "cold nuclear fusion." [1] Our research on muon catalysis led to several surprises. [2] A particularly striking discovery which remains partially unexplained is the smallness of the alpha-muon sticking probability ω_s. This quantity measures the likelihood of capture of the negative muon by the alpha particle which is synthesized during muon-induced deuteron-triton fusion. Table 1 juxtaposes experimental and theoretical results regarding this quantity, which has been shown to be the key factor which limits the muon catalysis yield. [3]

Table 1. Values for Alpha-muon Sticking (ω_s) in Liquid Deuterium Tritium Mixtures

	Theory	Experiment	Ref.
1981	0.9%		5
1984		$(0.4 \pm 0.1)\%$	4
1985	0.7%		9
1986		$(0.37 \pm 0.07)\%$	1
1987		$(0.45 \pm 0.05)\%$	6
1987		$(0.42 \pm 0.07)\%$	7
1987		$(0.42 \pm 0.14)\%$	8
1987	$(0.54 \pm 0.06)\%$		10

Our 1984 measurement showed that $\omega_s = (0.4 \pm 0.1)\%$ in liquid deuterium-tritium mixtures [4], which is much smaller than the calculated value of 0.9% [5] accepted before the experiment. Naturally, our surprising result was greeted with considerable skepticism. Indeed, preliminary results from an other experiment showed a value close to the existing theoretical prediction. We carefully scrutinized the result and repeated the measurement, publishing in 1986 the final result $\omega_s = (0.37 \pm 0.07)\%$. [2]. Subsequently, other groups [6,7,8] confirmed the result as shown in Table 1. Theoretical values were also reduced by about 30% [9], but still today the calculations significantly exceed the experimental values. [2,10]

Our recent observation of what appears to be a fusion phenomenon occurring at low levels in metals without muon catalysis [11] is likewise very surprising and has been greeted with understandable skepticism. We have repeated and extended the experiments at BYU, at the Gran Sasso Laboratory in Italy, and now at the Los Alamos National Laboratory, and continue to find strong evidence for a small but intriguing new effect. Our results will be reviewed here and in accompanying papers. [12]

At this stage, careful scrutiny and further work are clearly called for. In particular, our results imply that the "excess heat" claimed by others in superficially similar experiments [13] is not in fact due to fusion: the fusion product source rate is far too small to yield net power. It is also important to note that we lack a consistent theoretical model to account for cold fusion in condensed matter even at the low levels implied by our results. (Cinderella's shoe is absent, and some wonder about Cinderella.) Indeed, our early treatise on the

possibility of "piezonuclear fusion" without muons was rather pessimistic [14], although those musings led to our present studies.

Our experiments continue to show a small but meaningful effect which we hope to understand. We have come to expect the unexpected in cold nuclear fusion research. It may be too early to say that the latest surprising observation [11] has been confirmed, but there are encouraging signs. Hopefully this study will contribute to efforts to fit the puzzle pieces together.

Detection of Cold Fusion Neutrons

We have observed neutron production during low-voltage electrolytic infusion of deuterons into titanium or palladium electrodes. The fusion reaction

$$\mathrm{d} + \mathrm{d} \rightarrow {}^3\mathrm{He}(0.82\,\mathrm{MeV}) + \mathrm{n}(2.45\,\mathrm{MeV}) \qquad (1a)$$

is evidently catalyzed as d^+ and metal ions from the electrolyte are deposited on (and into) the cathode. Neutrons having approximately 2.5 MeV energy are detected with a sensitive neutron spectrometer. The experimental layout is portrayed in Figure 1 and a photograph of the apparatus in a recent configuration is shown in Figure 2. We have not yet obtained firm results regarding the parallel reaction

$$\mathrm{d} + \mathrm{d} \rightarrow \mathrm{p}(3.02\,\mathrm{MeV}) + \mathrm{t}(1.01\,\mathrm{MeV}) \qquad (1b)$$

as this requires different measuring procedures, although we have begun testing for tritium. However, it can be presumed that the reaction ($1b$) occurs at a nearly equal rate as the reaction ($1a$), as demonstrated in room-temperature muon-catalyzed fusion experiments [15].

The neutron spectrometer, developed at Brigham Young University over the past few years [16] in large measure for these experiments, has been central to the identification of the "cold fusion" process. The detector consists of a liquid organic scintillator (BC-505) contained in a glass cylinder 12.5 cm in diameter in which three lithium-6-doped glass scintillator plates are embedded. Neutrons deposit energy in the liquid scintillator via multiple collisions and the resulting light output yields energy information. These now low-energy neutrons are then scavenged by lithium-6 nuclei in the glass plates where the reaction $n + {}^6\mathrm{Li} \rightarrow t + {}^4\mathrm{He}$ results in scintillations in the glass. Pulse shapes from the two media differ so that distinct signals are registered by the two photomultiplier tubes (whose signals are summed). A coincidence of signals from the two media within 20 μsec identifies incoming and stopping neutrons.

An energy calibration of the spectrometer was obtained using 2.9 and 5.2 MeV neutrons, generated via deuteron-deuteron interactions at 90° and 0°, respectively, with respect to the deuteron beam from a Van de Graaff accelerator. The observed energy spectra (see Figure 3) show broad structures which imply that 2.5 MeV neutrons should appear in the multi-channel analyzer spectrum in channels 45-150. Note that the response of the liquid scintillator is linear for head-on neutron-proton collisions, allowing us to establish a relation for the high-energy shoulder seen in the energy distributions. The response is, however, non-linear for multiple collisions, resulting in a broad distribution ($\sim$ 50% FWHM) as seen in Figure 3. Stability of the detector system, including photomultiplier

tubes, was checked between data runs by measuring the counting rate for fission neutrons from a broad-spectrum californium-252 source.

We have performed extensive tests proving that our neutron counter does not respond preferentially in the 2.5 MeV pulse height range to background sources of radiation such as thermal neutrons. We tried to generate false 2.5-MeV neutron 'signals' by using various γ-ray and fission neutron sources and by turning on and off auxiliary equipment (including a large noisy compressor) without seeing any structure like that observed in neutron calibration runs. Neutron-producing machines such as the Van de Graaff accelerators were completely inactive during all foreground and background runs.

Background runs were made using operating cells (described below) containing standard electrodes and electrolytes, except that H_2O replaced the D_2O: numerous light-water control runs were performed prior to submission of our paper [11]. Other background runs were made using both new and previously used standard cells containing D_2O plus the usual electrolyte but with no electrical current. In the case of used D_2O cells, the current had been off for many hours. The individual background runs followed the featureless pattern of the integrated background illustrated in Figure 4. Observed background rates in the neutron counter are $\sim 10^{-3}$ s^{-1} in the energy region where 2.5 MeV neutrons are anticipated. By comparing energy spectra from γ-ray and neutron sources we have determined that approximately one-fourth of the observed background events arise from accidental coincidences of γ-rays and three-fourths from ambient neutrons. The γ-ray background comes mainly from radioactive radium and potassium in the surrounding materials. We attribute the ambient neutrons to cosmic-ray sources.

Cosmic ray backgrounds are a possible source of error in the experiment, although background runs show no evidence of structure at 2.5 MeV (see Figure 4). We performed Monte-Carlo [17] studies of neutron production in source media (primarily the shielding surrounding the detector) and found that moderation in these materials will wash out any structure in the neutron evaporation spectrum and produce a smoothly decreasing background energy spectrum above 0.5 MeV. We have also reviewed cosmic-ray neutron flux data [18] for the period of the experiments; no correlations between fluctuations and observed neutron signals were found.

Is it possible that cosmic-ray muons could lead to muon-catalyzed fusion at significant levels in the electrochemical cells? Certainly not! Studies of muon-catalyzed fusion demonstrate that muon-scavenging by $Z > 2$ materials is very rapid [19]. Suppose that a cosmic-ray muon stops in D_2O or in a deuterated metal. Even if a dμ atom should form first, muon transfer to a metal or oxygen nucleus will proceed at an enormously greater rate than formation of a ddμ molecule, preventing muon-catalyzed fusion. Indeed, in our muon catalysis experiments we find that impurities in deuterium must be kept at the parts-per-million level in order to observe d-d fusion.[20] In the electrochemical cell, the "contamination" level is a million times too large to permit observation of muon-catalyzed fusion.

Prescription for Electrolytic Cells

During the search for suitable catalytic materials, we developed the following prescription for the electrolytic cells. The electrolyte is a mixture of ~160 g deuterium oxide (99.8% D_2O) plus various metal salts in ~0.1 g amounts each: $FeSO_4 \cdot 7H_2O$, $NiCl_2 \cdot 6H_2O$,

$PdCl_2$, $CaCO_3$, $Li_2SO_4 \cdot H_2O$, $NaSO_4 \cdot 10H_2O$, $CaH_4\ (PO_4)_2 \cdot H_2O$, $TiOSO_4 \cdot H_2SO_4 \cdot 8H_2O$, and a very small amount of gold cyanide (two drops of solution). The pH is adjusted to pH $\lesssim 3$ with HNO_3. The clear solution is decanted from this brew and used as the electrolyte. Our electrolyte compositions evolved from geochemical considerations, and changed as results were observed. Now that our exploratory searches have disclosed a small piezonuclear fusion effect, it remains to disentangle the relevant components. Owing to the complex electrochemistry inherent in these cells, our recent work has focussed on loading deuterons into metals using pressurized deuterium, as we will discuss later (see also Ref. 12).

Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective cathode materials. Individual electrodes consisted of approximately 1 g purified "fused" titanium in pellet form, or 0.05 g of 0.25 mm thick palladium foils (surface roughened with abrasive) or 5 g of mossy palladium. The fused titanium is a very porous material having the appearance of tiny Ti crystals fused or sintered together, obtained from Fisher Scientific Company. The mossy palladium likewise has a large surface area-to-volume ratio. Since we suspect that the phenomenon may occur on or near the surface of the cathode, cathodes having very large surface areas constitute an integral part of our prescription.

Gold foil was used for the positive electrodes, and gold ions were found to enter the solution and to be deposited on the cathode surface. DC power supplies provided 3-25 volts across each cell at currents of 10-500 mA. Six volts at 50-100 mA in each cell was typical. Correlations between neutron yield and voltage, current density, or surface characteristics of the metallic cathode have yet to be sorted out.

Small jars, approximately 4 cm high $\times$ 4 cm diameter, held $\sim$ 20 ml of electrolyte solution each. In general, 4-8 electrolytic cells were used simultaneously in each ~eight-hour run. The cells were placed on or alongside the neutron counter, as shown in Figure 1. The cells are doubtless far from optimum at present. Nevertheless, the present combination of simple electrolytic cells with the neutron spectrometer does provide evidence for a new phenomenon of cold nuclear fusion during deuterium infusion into metals.

Fusion Rate Determination

Figure 5 displays, for each data run, the foreground count rate in the 2.5 MeV-energy region minus the corresponding background rate (R_i). The error bars ($\pm\sigma_i$) reflect the combined statistical uncertainty of foreground and background count rates. For this meeting, I am responding to a request that we present our data [11] in terms of source rates rather than foreground-to-background ratios. The neutron-detection efficiency of about 1%, which varied somewhat after run 8 due to replacement of bad electronic modules and tuning is reflected in the presentation of *source* rates in Figure 5.

Run 6 is particularly noteworthy, having a statistical significance of approximately 5 standard deviations above background. Fused titanium pellets were used as negative electrodes with a total mass of about 3 g. We observed that the neutron production rate increased after about one hour of electrolysis. After about eight hours, the rate dropped dramatically as shown in the follow-on run 7. At this time, surfaces of the Ti electrodes showed a dark gray coating. An analysis using electron microscopy with a microprobe showed that the surface coating was mostly iron, deposited with deuterons at the cathode.

The same phenomenon of having the neutron signal drop after about eight hours of

operation appears in run 13 followed by run 14 both using the same eight electrochemical cells. As in run 6, the negative electrodes developed coatings after a few hours of electrolysis. These observations suggest the importance of surface conditions on the cold fusion process. Indeed, wide variations in surface conditions are anticipated in operating electrochemical cells with numerous ionic species, and such variations may account in part for the fluctuations in the signal level which are evident in Figure 5. The observed "turning off" of the signal after $\sim$ 8 hours may account for a low signal rate in runs 1 and 3, in that a few-hour signal may have been overwhelmed after a long ($\sim$20 hour) running time. When run 10 started with rates substantially above background, we stopped the run and removed half of the electrochemical cells as a test. The neutron production rate dropped off as expected (run 11).

To estimate the neutron source rate, we might exclude runs 1, 3, 7, 11 and 14, since there are systematic reasons for their low foreground-minus-background rates as discussed above. The mean neutron production rate and its uncertainty are determined using data in Figure 5 and the expressions:

$$\text{mean value } \mu = \left(\sum \frac{R_i}{\sigma_i^2}\right) \Big/ \left(\sum \frac{1}{\sigma_i^2}\right), \tag{2a}$$

$$\text{and } \sigma^2_{<\mu>} = \left(\sum \frac{(R_i - \mu)^2}{\sigma_i^2}\right) \left(\frac{1}{n-1}\right) \Big/ \left(\sum \frac{1}{\sigma_i^2}\right) \tag{2b}$$

The result is that the mean 2.5 MeV-neutron production rate is:

$$\mu = (0.14 \pm 0.03)\, \mathrm{s}^{-1}. \tag{3}$$

I emphasize that the result of relation (3), is based on ALL the data acquired through March 6,1989, if we include in our prescription the criterion that the run duration be limited (eight hours maximum was used here). This specification is important since we have observed the neutron production rate to drop after a few hours of running, as explained above, perhaps due to plating-out of metal ions on the cathode surface. After observing the drop-off effect, we nevertheless ran one "test" of a few days duration to look for long-term effects. Since the data were accumulated using a multi-channel analyzer, we could not extract just the first eight hours of data according to our prescription. Consequently, this very long run is not included above. We have given preliminary consideration to other metals including nickel, iron, copper, zirconium and lithium-aluminum hydride. We also performed tests with a neutron flux monitor and with a NaI detector beginning in 1986. Encouraging hints from these early data prompted development of the neutron spectrometer used for our current experiments. Note also that we do *not* specify a long period of time for "charging" the cathode, although we have seen the neutron production rate increase after about one hour of cathode conditioning.

Incorporating the latest data acquired at BYU and at the Gran Sasso laboratory, using relations (2), we find a mean 2.5 MeV neutron production rate of $\mu = (0.14 \pm 0.025) s^{-1}$. The statistical uncertainty reflects the fact that the source rate is observed to vary significantly (and inexplicably) between runs equation (see equation 2b). We find the same effect in data acquired at Los Alamos as shown in Figure 6. For this experiment we have added deuterium

gas at about 50 atm to mixed titanium alloys in a cylinder approximately 5 cm diameter by 25 cm long. [See also contribution of H. Menlove et al. in this volume.] The experiment ran at constant room temperature, and the pressure changed very little as deuterium slowly permeated into the metals. Nevertheless, the neutron-singles production rate varies significantly between 4000-second sampling periods. While puzzling, this effect appears consistent with the rate variations shown in Figure 5 for the electrochemical approach.

As an example, the cold nuclear fusion rate during electrolytic fusion is estimated using run 6 (see Figure 5) as follows:

$$\text{Fusions per deuteron pair} = \left(\frac{R}{\epsilon}\right) \Big/ \left(M \times \frac{d}{2M}\right) \tag{4}$$

where the observed fusion rate $R = (4.1 \pm 0.8) \times 10^{-3}$ fusions/s, the neutron detection efficiency, including geometrical acceptance, is calculated using a monte carlo neutron-photon transport code [17] to be $\epsilon = (1.0 \pm 0.3)\%$; $M \simeq 4 \times 10^{22}$ titanium atoms for 3 g of titanium; and the deuteron-pair per metal ion ratio $\frac{d}{2M} \approx 1$ is based on the ***assumption*** that nearly all tetrahedral sites in the titanium lattice are occupied, forming the $\gamma-TiD_2$ hydride. Then the estimated cold nuclear fusion rate per deuteron pair by equation (4) is

$$\lambda_f \sim 10^{-23} \text{ fusions/deuteron pair/second.} \tag{5}$$

If most fusions take place near the surface, or if the titanium lattice is far from saturated with deuterons, or if conditions favoring fusion occur intermittently, then the inferred instantaneous fusion rate could be much larger, perhaps 10^{-20} fusions/d-d/second.

Owing to the questionable assumptions involved in the normalization to deuteron pairs (equation 5), it is sensible to express the yield in terms of source neutrons per second as we did in equation 3 along with a specification of the cathode mass and porosity. In fact, it may be better to normalize to the total surface area rather than the mass of the cathodes. But the estimated rate normalized to deuterons (equation 5) is still useful for order-of-magnitude comparisons with hypothesized geological cold fusion rates.

Observation of Neutron Bursts

Our collaborative research at Los Alamos has disclosed that neutrons are produced in substantial numbers in very short bursts, in both electrolytic cells and in titanium alloys subjected to pressurized deuterium gas. (Both methods also produce random neutron-singles.) These results will be discussed in some detail by H. Menlove of Los Alamos [12]. Here I wish to emphasize that the results appear to be rather independent of the method used to drive deuterons into the metal. Furthermore, we observed bursts of neutrons for a few hours after the current was turned off in the electrolytic cells, indicating that the electric field in the cells is not essential to whatever produces the neutrons. However, we have not observed neutron production in materials which have been fully deuterided and are at equilibrium; we conclude that non-equilibrium conditions are essential.

Burst results for electrolytic cells are shown in Figure 7. The six electrolytic cells used for these results contained sintered titanium-palladium cathodes along with V and Zr foils. The electrolyte consisted of D_2O plus 10 g/l Li_2SO_4 with the pH adjusted to 4 with H_2SO_4. The voltage was varied between 4.4 (0.4 A) and 9.9 volts (1.6 A).

We note that there are small bursts even in the background run, which we attribute to neutron evaporation induced by cosmic ray (principally muon) capture on nuclei. We observe that correlated neutron counts of a dozen neutrons (correcting for the 19% efficiency of the detector) are not uncommon in the background. Much larger bursts, some of the order of 100 neutrons in a 50 microsecond window, occur with the electrolytic cells.[Fig. 7] This represents a striking phenomenon worth studying: such a large number of neutrons in a micro-burst is very difficult to account for. It could be a fluke in the detector, but we have seen $\sim 10^2$ neutron bursts in different detectors, using both electrochemical and D_2 gas-pressure methods. Furthermore, the detectors ran for several weeks before our experiments began providing a long history without large burst events. Cosmic-ray capture on a single nucleus could not liberate the hundreds of neutrons seen in some bursts. We surmise that some new fusion mechanism is at work here.

The experimental evidence is too sparse to provide much guidance to theoretical model development at present. One possible explanation is that strong electric fields are generated as a crack propagates in the metal deuteride. Deuterons could be accelerated in the fields to keV energies, and collisions could then result in "hot" fusion.[21] (This process has been dubbed "fractofusion"; see contribution to this meeting by R. Ryan et al.) My own theory is that fusion is induced during violent rearrangement of the lattice accompanying phase changes, due to variations in temperature or the deuteron/metal ion ratio. In this view, impurities and phase boundaries in the lattice may be important as fusion sites. Shock compression of deuterons embedded in the lattice may lead to fusion.

To make progress, we need to measure and compare p-d, d-d and d-t "cold fusion" rates. Prepartions are underway to do just this at Los Alamos. We also need to assess the time-structure of the bursts. It is safe to say that a great deal of work remains to first confirm that the observed effects are due to fusion, then to model and control the phenomenon. Only then can one seriously talk about applications (such as a monoenergetic neutron source).

Neutron Energy Spectrum

Our fusion measurements have been analyzed in several ways, all of which indicate a small but significantly meaningful emission of neutrons. There may be some who wonder if there is a possibility that the analyses are not correct or something has been overlooked since the signal is so small. Thus we present here another method of analysis of the data published in Nature.[11] This method points out the elegance of the use of an energy-sensitive spectrometer in such measurements.

We start by fitting the summed background distribution from channels 16 to 420 inclusive, binned in three channels per bin, with the simple function:

$$y = \frac{A}{x^2 + Bx + C} \tag{6}$$

where x is the bin number and y represents counts in bin x.

The values of B and C vary the shape of the curve and A represents an amplitude or scaling factor. Channels 1-15 are ignored because of the artificial cut-off at low energies; the long high-energy tail is also truncated. The values giving the best least-squares fit to the background data are: $A = 61100 \pm 4600$, $B = 46.2 \pm 4.9$, and $C = 198 \pm 17$. The result

is plotted in Fig. 4. The χ^2 for the fit is 137 with 132 degrees of freedom, indicating an excellent fit to the background data.

Assuming that a monoenergetic neutron signal should form a Gaussian structure above the background, we fit the foreground data (binned as before) with the function:

$$y = \frac{A}{x^2 + 46.2x + 198} + D\exp\left[\frac{-(x-p)^2}{w}\right] \tag{7}$$

where we kept the background-shape parameters B and C constant. The best non-linear fit yielded a χ^2 of 103 with 131 degrees of freedom which is an excellent fit to the data, shown in Fig. 8. We obtain $A = 27290 \pm 870$, $p = 27.3 \pm 5.2$, $w = 152 \pm 50$,and $D = 6.1 \pm 1.5$. The last term implies that the statistical significance of the neutron signal is over 4σ.

The center of the Gaussian is at channel 97 ± 16 with a full width at half maximum of 60 ± 11 channels. It is important that both the position and width of this feature are consistent with those expected for 2.5 MeV neutrons detected in the BYU neutron spectrometer, based on calibrations with neutrons of known energy. This is a crucial result, adding considerably to our confidence that the feature represents real neutrons, generated by d-d fusion (reaction 1).

We note here that we cannot clearly distinguish in these data whether the neutrons were produced randomly or in bursts. By examining the correlation coefficient between the parameters A and D in equation 2, we find that an increase in the background of 5σ would be required to remove the significance of the Gaussian peak. Analyzing the data in different ways, we conclude that the neutron count at 2.5 MeV is statistically significant and highly suggestive of low-level deuteron-deuteron fusion under unusual conditions.

The Geological Cold Fusion Hypothesis

Our observation of d-d fusion near room-temperature in the laboratory may have profound geophysical implications. We make the hypothesis that thermal effects in the earth and the distribution of ^{3}He can be explained in part by the fusion reactions (1) and

$$\mathrm{p} + \mathrm{d} \rightarrow {}^3\mathrm{He} + \gamma(5.4\,\mathrm{MeV}). \tag{8}$$

Deuterium was incorporated in the earth during its formation. The current abundance in sea water is about 1.5×10^{-4} deuterons per proton. Water is carried down into the earth's upper mantle at converging plate margins, and seawater is transported as deep as the Moho at spreading regions [22]. Estimates of water subduction suggest that a water mass equal to the ocean mass is cycled through the mantle in about 1-billion years [22]. Thus, 1.4×10^{43} deuterons are cycled through the mantle in 3×10^{16} sec. Since each p-d fusion releases 5.4 MeV (8.6×10^{-13} J), we calculate that a heat flux of 750 mW/m^2, averaged over the earth, would result if all deuterium fused at the rate at which it is supplied by subduction. This is more than ten times the estimate of the actual flux of 60 mW/m^2 [23]. Thus, geological cold fusion could in principle contribute to the observed heat flux and the high temperature of the earth's core, and provide an energy source for plate tectonics.

The foregoing data allow a geological fusion rate constant λ_f to be estimated. We assume a first-order rate equation for p-d fusion: $\mathrm{dN} = \lambda_f\ \mathrm{N}\ \mathrm{dt}$, or $\lambda_f = (\mathrm{dN/N})/\mathrm{dt}$. The

fraction (dN/N) is the ratio of the number of fusions which take place to the number of atoms available. It is also the rate of fusion divided by the rate of supply of deuterons; thus, dN/N is equal to the actual heat flux from the earth divided by the possible heat flux so that

$$\lambda_f = (60/750)/3 \times 10^{16}\mathrm{s} = 3 \times 10^{-18}\mathrm{s}^{-1}. \tag{9}$$

While the earth's heat must certainly derive from several sources, "cold" geological nuclear fusion could account for steady-state production of considerable heat and ^{3}He in the earth's interior.

High values of the ^{3}He/^{4}He ratio are found in the rocks, liquids, and gases from volcanoes and other active tectonic regions [24]. Primordial ^{3}He will be present from the formation of the earth [24], but some may be generated by terrestrial nuclear fusion. To form one more estimate for a possible rate of fusion in the Earth, we assume a steady-state model in which the known flux of ^{3}He out of the mantle, 2×10^{19} ^{3}He atoms per second [25], arises from p-d fusion occurring uniformly in the mantle water reservoir, taken as $\sim 1.4 \times 10^{24}$ g (R. Poreda, personal communication). As each p-d fusion produces one ^{3}He atom, and as the isotopic abundance of deuterium in water is $\sim 1.5 \times 10^{-4}$ deuterons per proton, we infer a geological fusion rate constant, λ_f, of

$$\begin{aligned} \lambda_f &\approx \frac{2 \times 10^{19}\ ^3\mathrm{He\ atoms\ s}^{-1}}{1.4 \times 10^{43}\ \mathrm{deuterons}} \\ &\approx 10^{-24}\ \mathrm{fusions\ d}^{-1}\mathrm{s}^{-1} \end{aligned} \tag{10}$$

Note that if the Earth contains 'primordial' ^{3}He, our estimated rate will be an upper limit; on the other hand, if fusion-produced ^{3}He is stored in the mantle (so that the outward flux does not equal the production rate), our value will be a lower limit. The discovery of cold nuclear fusion in the laboratory, with a rate constant within a few orders of magnitude of that estimated from geologic data, supports our hypothesis that cold fusion may occur in the earth.

Based on this model, we predict that some tritium should be produced by d-d fusion in the earth (see equation 1*b*). Since tritium decays according to $t \rightarrow {}^3\mathrm{He} + \beta + \nu$ with a 12.4-year half-life, detection of tritium in volcanic emissions would corroborate our hypothesis of geological cold fusion in recent times.

Consider the following evidence. A tritium-monitoring station was operated at Mauna Loa on Hawaii island from August 1971 to the end of 1977; we have found strong correlations between tritium detected at Mauna Loa and nearby volcanic activity in this period of time. Figure 9 displays data compiled by Ostlund for HT gas measured at the Mauna Loa station in 1972 [26]. Similar data taken at Miami, Florida, are provided for comparison. A striking spike in the tritium level is clearly seen in the February-March 1972 Mauna Loa data. Ostlund notes that these significant tritium readings over a several-week period have not been previously understood; in particular, the timing and shape of the peak is inconsistent with hydrogen bomb tests in the Soviet Union five months earlier [H.G. Ostlund, personal communication]. However, this interesting tritium signal is coincident with a major eruption of the Mauna Ulu volcano [27] 40 km to the southeast. Furthermore, winds

carried volcanic gases northwest, towards the Mauna Loa station and on towards Honolulu 200 km away; the weather report for this period in 1972 states: "Trade winds [from the northeast] were infrequent and the southerly flow that replaced them occasionally blanketed the state with volcanic haze from an eruption on Hawaii Island .... High particulate matter measurements in Honolulu confirmed the northward spread of haze from the Mauna Ulu Volcano eruption on Hawaii Island." [28]

This remarkable set of circumstances permits us to estimate the amount of tritium plausibly released during the February-March 1972 eruption of Mauna Ulu. Based on the distance to the Mauna Loa station and average 8 mph winds [28], we estimate that on average 100 curies of tritium were released per day for 30 days. An accidental release of this magnitude of man-made tritium sustained for several weeks is highly unlikely. We conclude that this volcanic eruption may have freed tritium produced by geological nuclear reactions.

Other HT data from the Mauna Loa station, such as the high readings in the latter half of 1972, are also coincident with volcanic activity, although a tritium-releasing bomb test also occurred in the Soviet Union in late August. A major spike in atmospheric HT observed near Hawaii in December 1974 - June 1975 [26] coincides with another large volcanic eruption on Hawaii Island, but the significance is again obscured by H-bomb tests. Finally, no significant deviations in HT readings are noted in 1976 or 1977 [26] when no volcanic activity is noted, except for "gentle" activity at Kileau on September 17, 1977 [29].

Cold nuclear fusion may be important in other celestial bodies besides earth. Jupiter, for example, radiates about twice as much heat as it receives from the sun [14]. It is interesting to consider whether cold nuclear fusion in the core of Jupiter, which is probably metallic hydrogen plus iron silicate, could account for its large heat production. Heat is radiated at an approximate rate of 10^{18} W, which could be produced by p-d fusions occurring at a rate of 10^{30} s^{-1} [14]. Assuming a predominately hydrogen core of radius 4.6×10^9 cm, having a density ~ 10 g/cm^3 and a deuteron/proton ratio of roughly 10^{-4}, we deduce a required p-d fusion rate of $\lambda_f \sim 10^{-19}$ fusions per deuteron per second—in reasonable agreement with cold fusion rates deduced for terrestrial conditions. If the cold-fusion hypothesis holds, we predict Jupiter to have a large ^{3}He/^{4}He ratio and a small d/p ratio (relative to other planets). There may even be detectable tritium on the planet. Interestingly, the Galileo space probe is already designed to measure these quantities in 1991.

Conclusions

We continue to observe low-level neutron production in metals during deuteride formation in experiments at BYU, at the Gran Sasso Laboratory, and at the Los Alamos National Laboratory. We attribute these observations to a new fusion phenomenon. [11] The random neutron source rate is approximately 0.1 neutrons per second, consistent in both electrochemical-cell and gas-pressure techniques. Bursts of $\sim 10^2$ neutrons in a ~ 50 μs interval are also observed using both techniques. The fusion rates are very low, with no net power produced. We conclude that "excess heat" claims should not be confused with the cold fusion phenomenon.

However, cold fusion is a potentially interesting phenomenon which merits further

study. It may be important to our understanding of fusion processes, of metal-hydrogen systems, and of geophysics.

This research is supported by the Advanced Energy Projects Division of the U.S. Department of Energy.

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FIGURE CAPTIONS

1. Schematic view of the experimental set-up. Electrolytic cells are shown on top of the neutron spectrometer.

2. Photograph of experimental apparatus, showing a D_2 -pressurized sample cylinder containing a titanium alloy (Ti 6-6-2) between two neutron detectors in a recent experiment at BYU. Electrolytic cells have been placed on top of one detector for the photograph.

3. The energy spectrum obtained with 5.2 MeV neutrons incident on the BYU neutron spectrometer.

4. Background data (histogram) and least-squares fit to the data (solid line) of the function: $y = A / (x^2 + Bx + C)$. Insert shows background data less least-squares fit.

5. Background-subtracted count rates in the 2.5 MeV region, corrected for neutron-detection efficiency, for each data run. Dashed lines represent runs which are expected to have lower rates as described in the text. Error bars reflect the combined statistical uncertainties of foreground and corresponding background count rates.

6. The observed neutron singles counting rate for Ti alloys contained in a stainless steel cylinder pressurized to about 50 atm with D_2 gas. The right-hand part of the figure corresponds to data taken with a dummy target. Both sets of data were taken at the Los Alamos National Laboratory using a counter comprising 16 helium-3 proportional counter tubes embedded in a polyethylene moderator, with a total neutron detection efficiency of 31%.

7. The time-correlated neutron counts versus time for six electrolysis cells, showing neutron burst events (vertical lines).

8. Foreground data (histogram), least-squares fit to the foreground (upper curve), and normalized background fit (lower curve). Insert shows foreground data less background fit.

9. Atmospheric tritium measured at the Mauna Loa, Hawaii, station in 1972 juxtaposed with volcanic eruption times of the nearby Mauna Ulu volcano in 1972. For comparison, tritium levels for the Miami, Florida station in 1972 and for the Mauna Loa station in 1976, when there was no volcanic activity, are also shown.

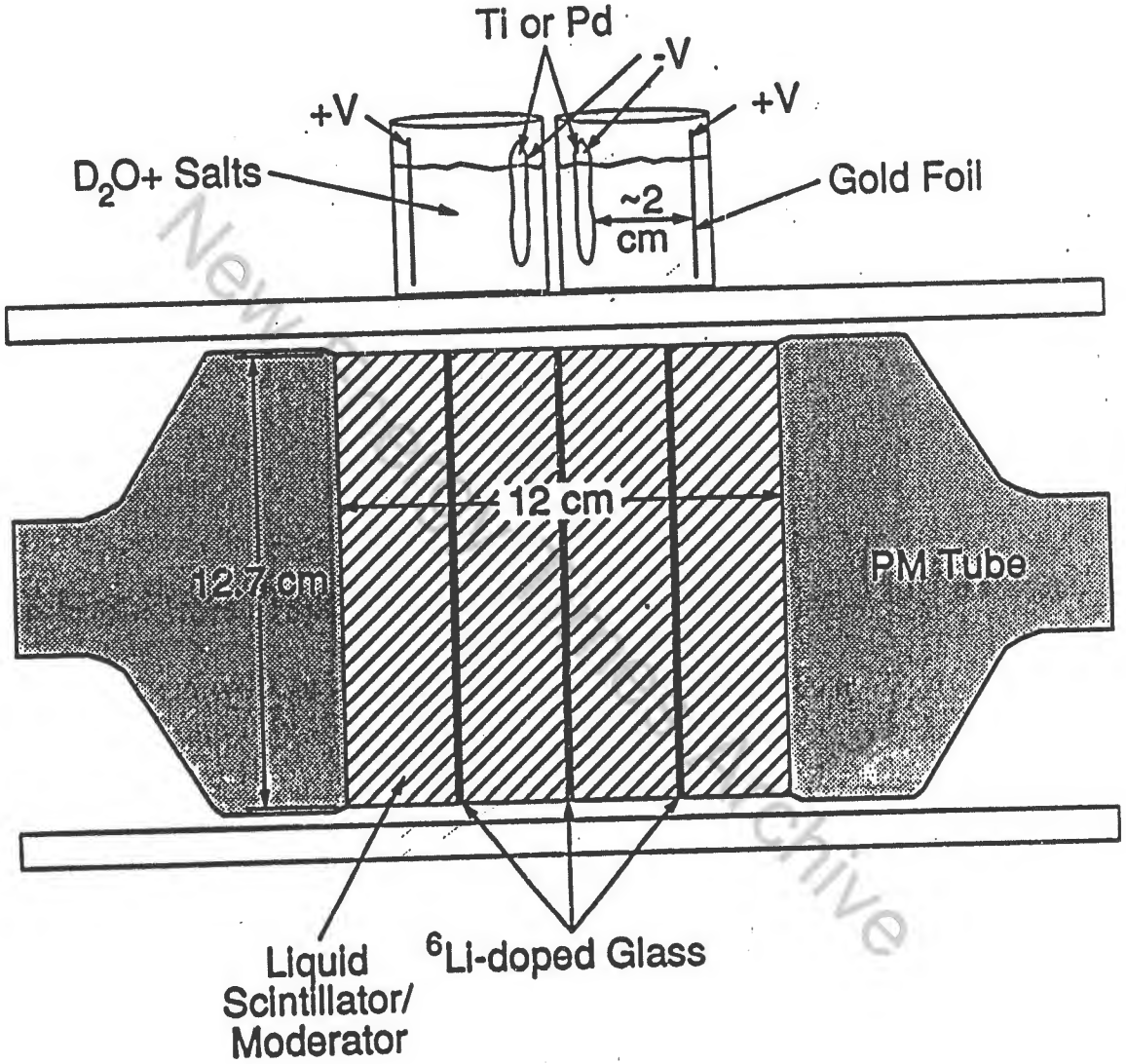
Ti or Pd
-V
+V
+V
D_2O+ Salts
Gold Foil
~2 cm
12 cm
12.7 cm
PM Tube
Liquid Scintillator/ Moderator
^{6}Li-doped Glass

Fig.1

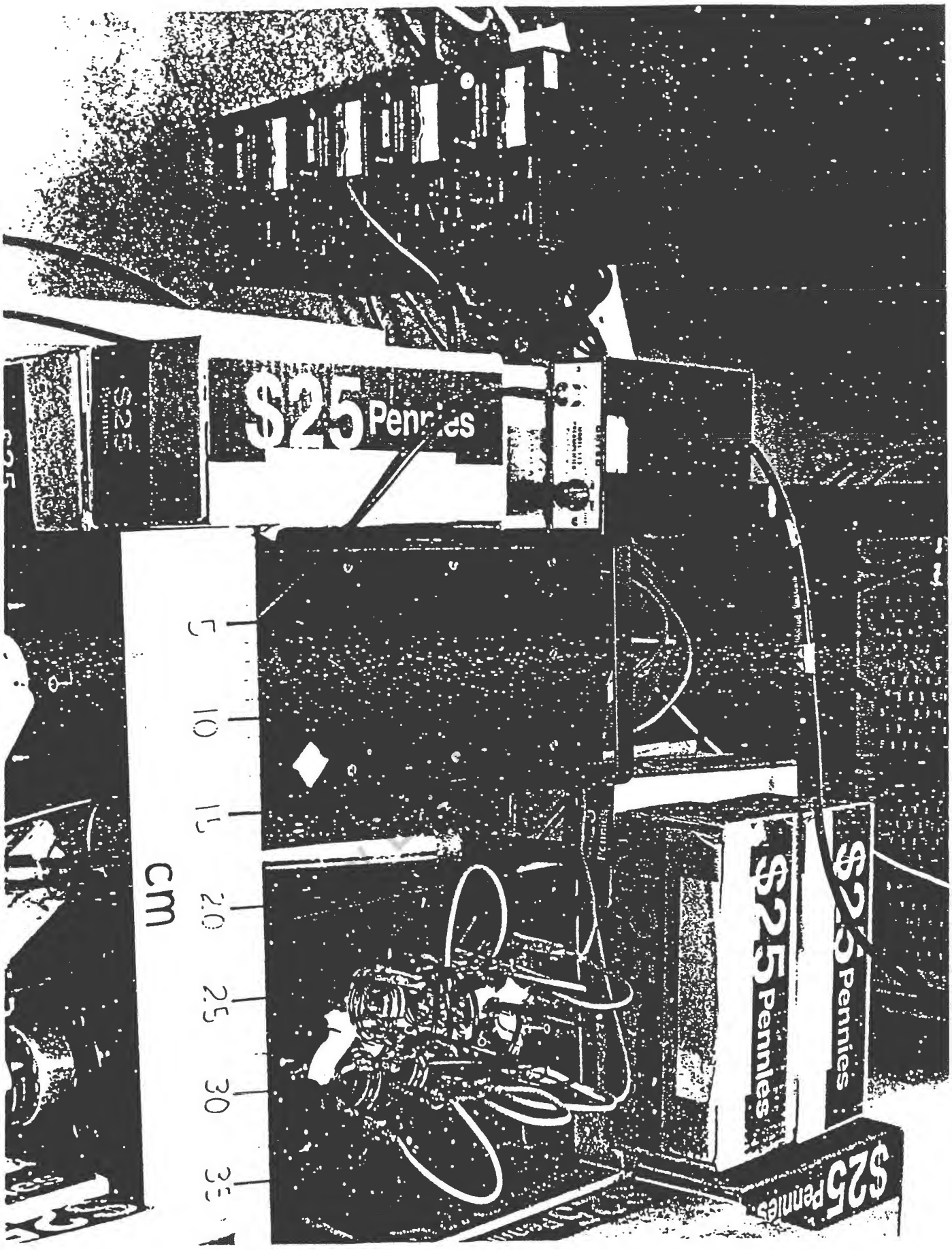

Fig 2 – Photo

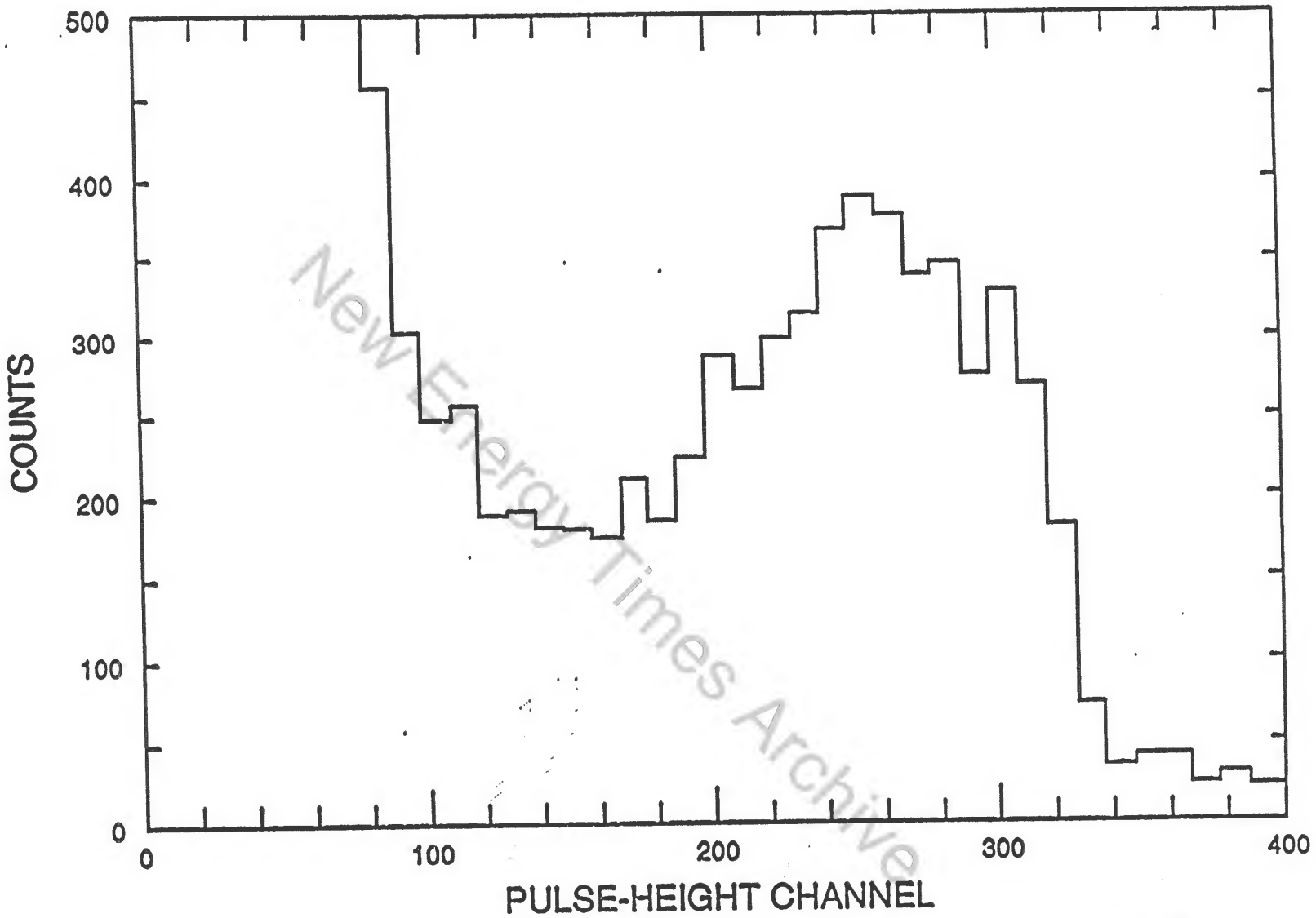

Fig. 3

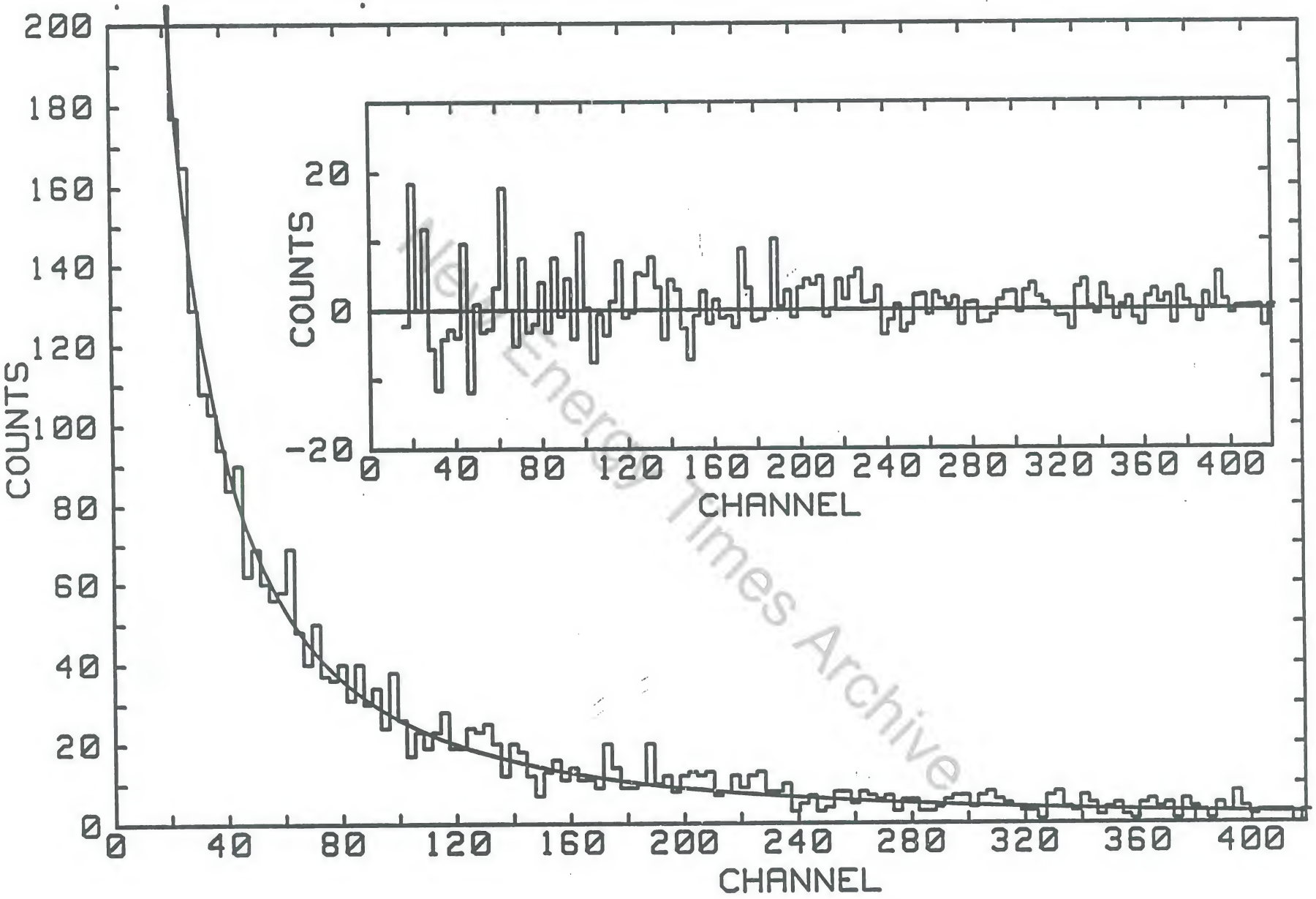

Fig. 4

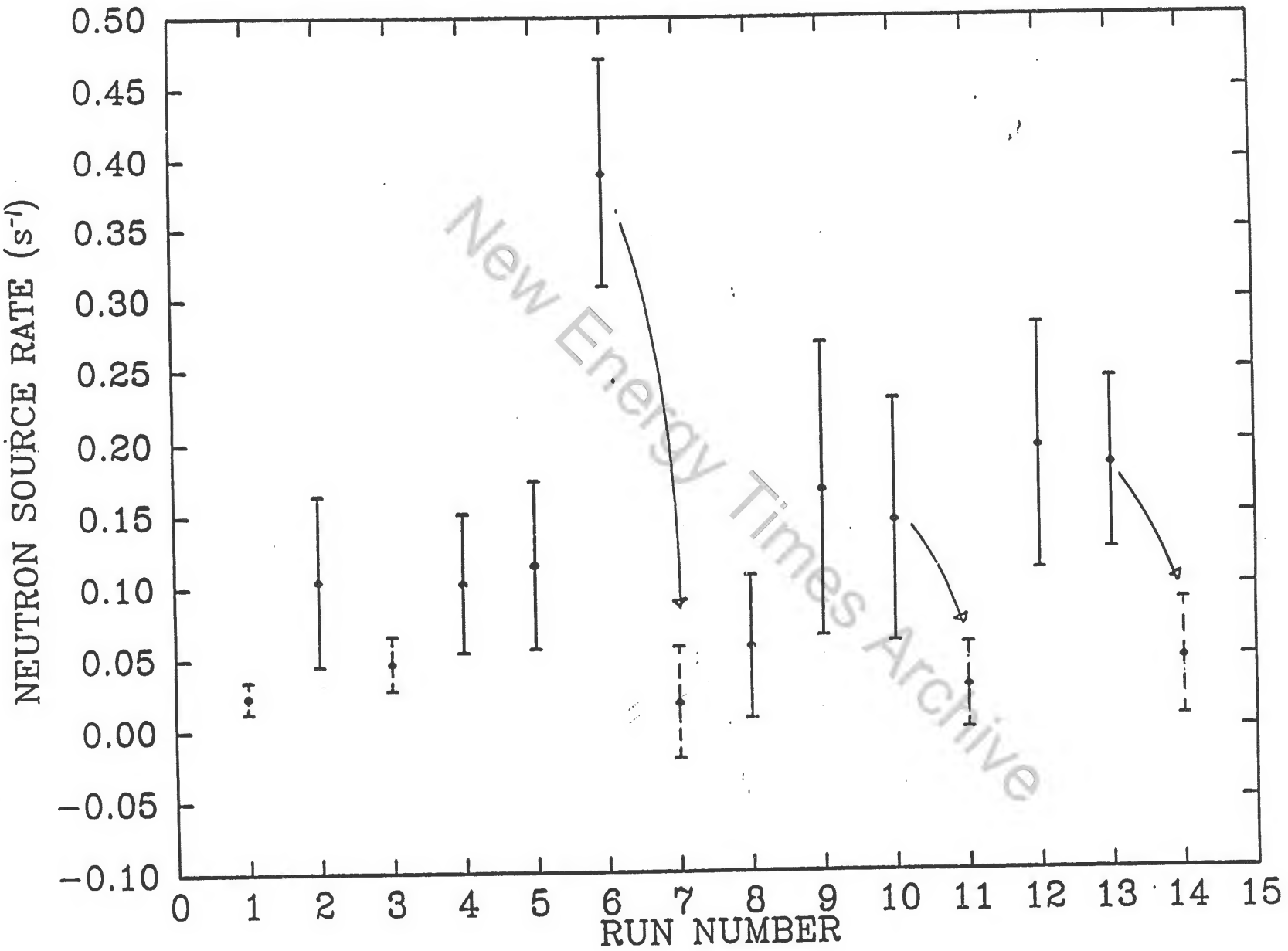

Fig. 5

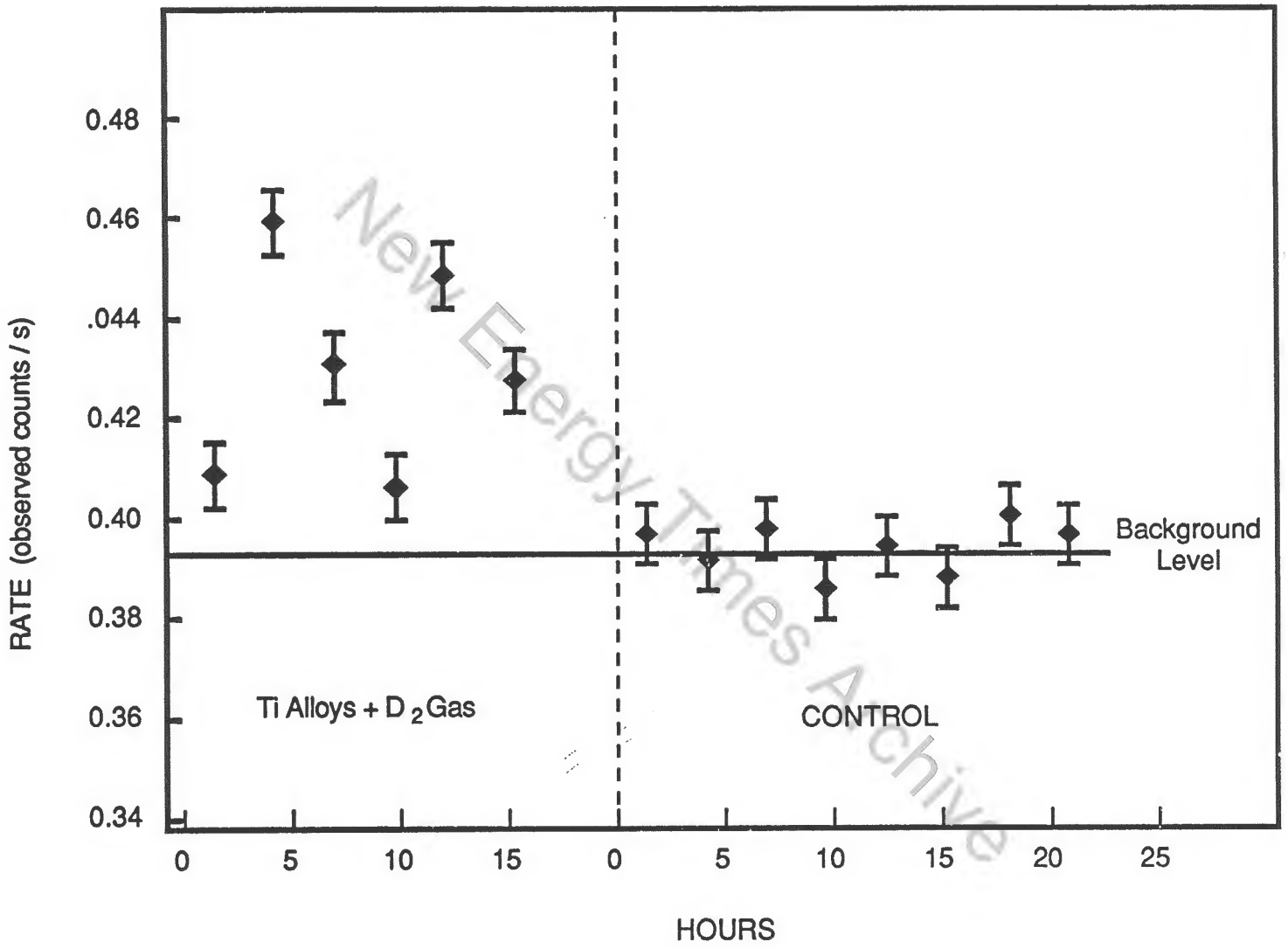

Fig. 6

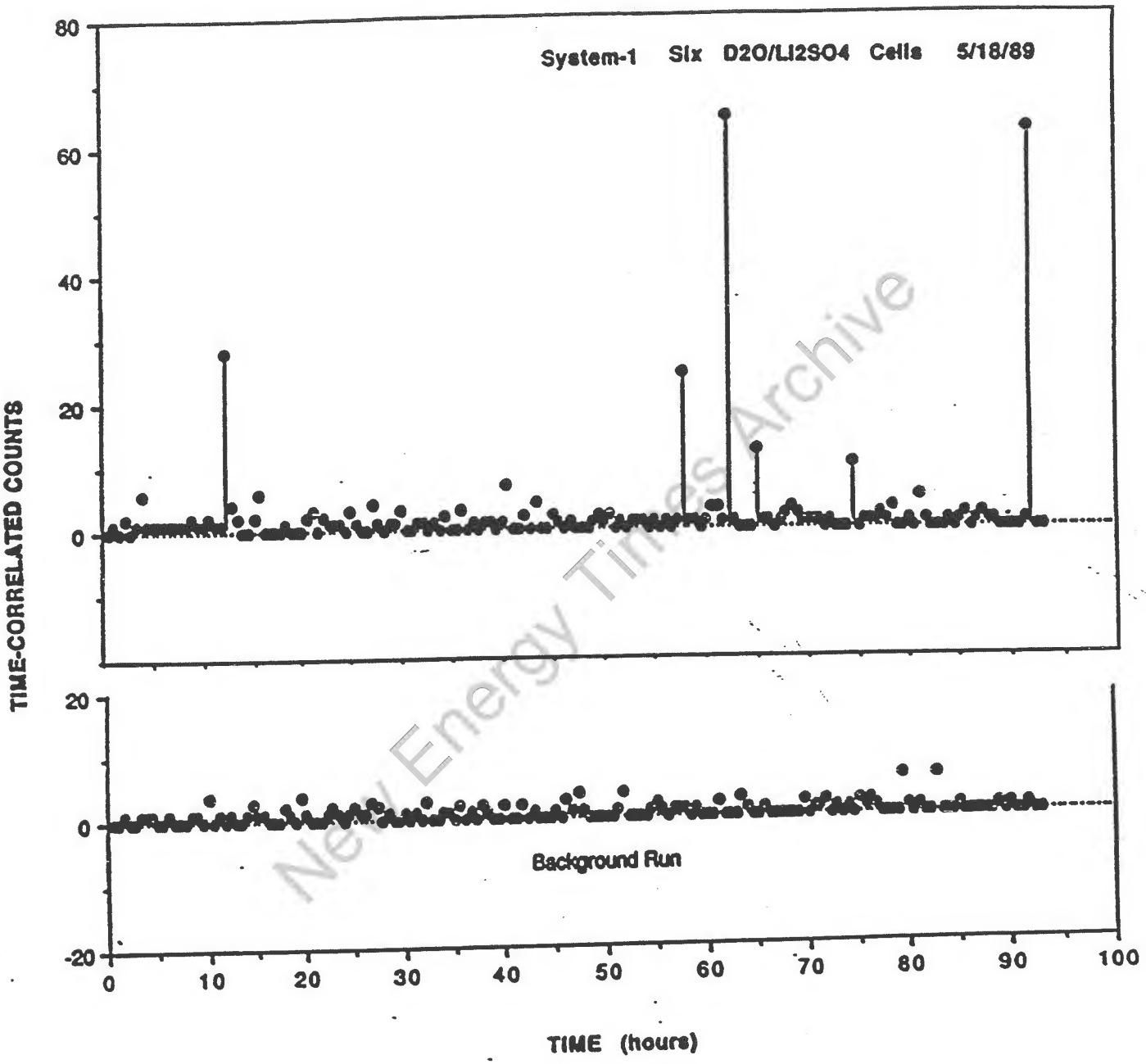

Fig. 7

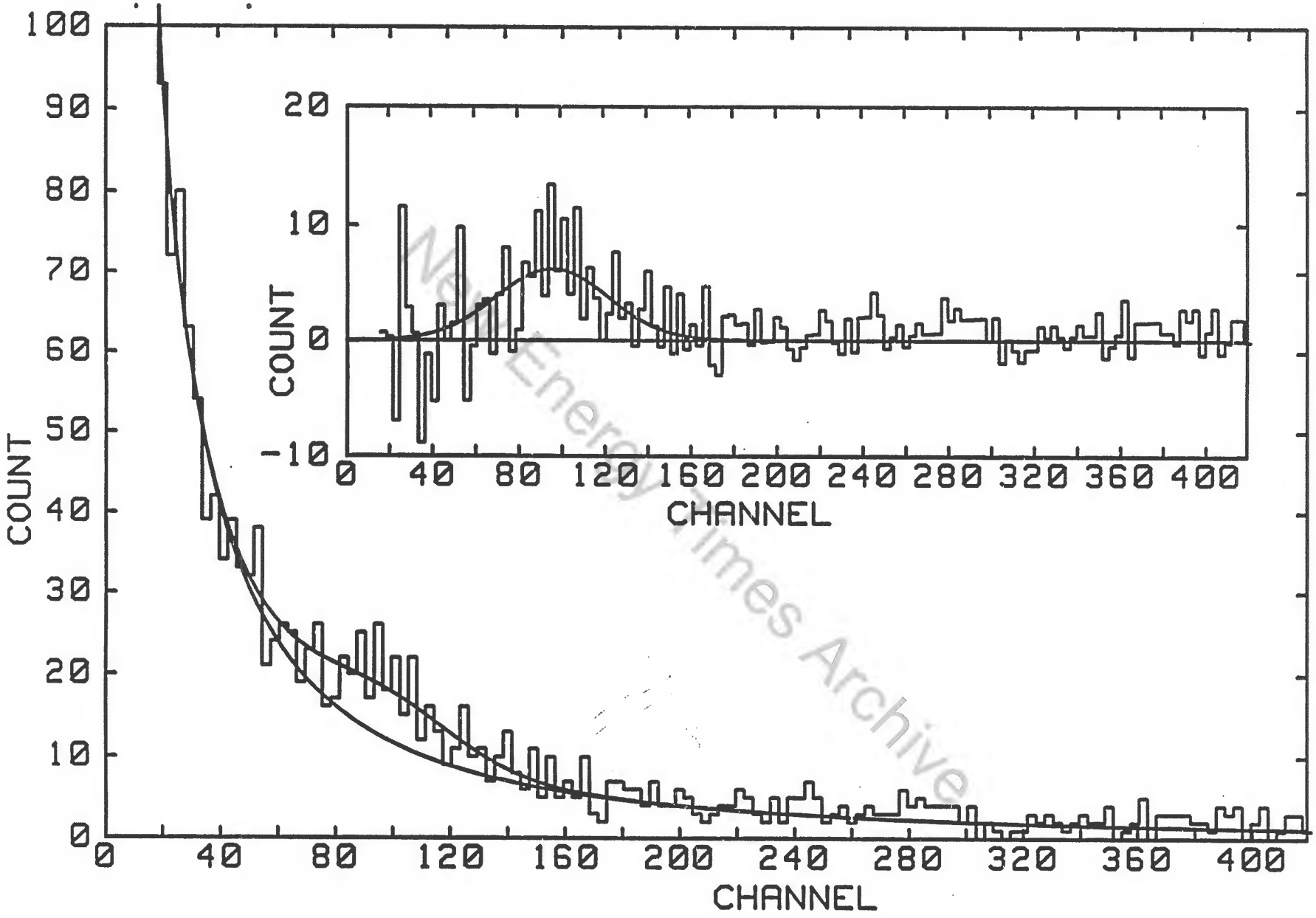

Fig. 8

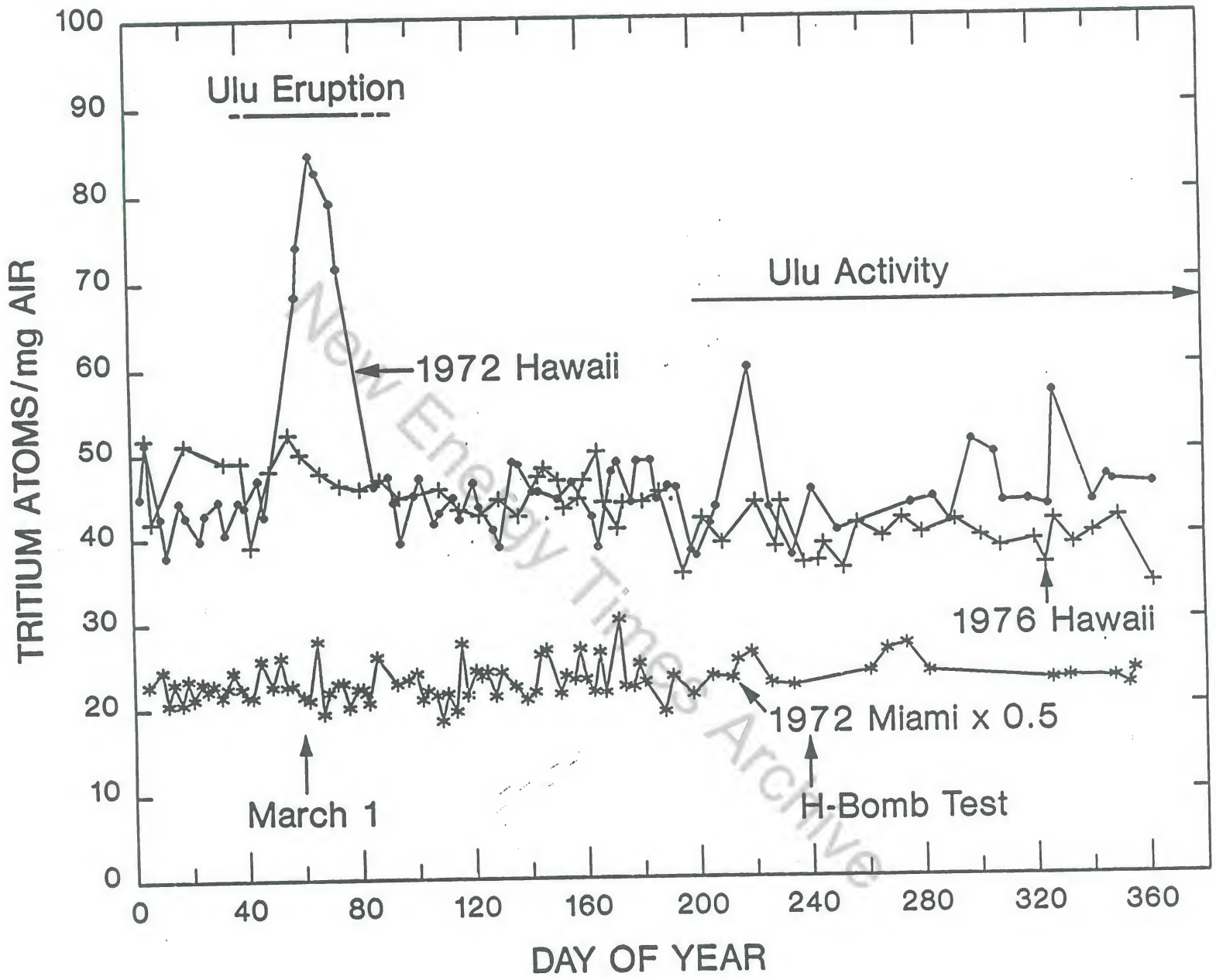

Fig. 9